THE

AMERICAN

MEDICAL RECORDER.

VOL. III.] OCTOBER, 1820. [NO IV.

ON THE MODUS OPERANDI OF BLOOD-LETTING.

Observations on the Modus Operandi of Blood-letting; with an inquiry into the propriety of abstracting Blood from different parts of the body, according to the nature and seat of the Disease. By Henry W. Ducachet, M. D. of New York.

IT has been said, with great truth, that the practice of medicine is regulated by fashion. This observation has, in no instance, been more remarkably verified, than in the subject of which we are to treat. There was a time, when the different parts of the body from which blood was to be drawn, were designated with the most scrupulous minuteness; and prescribed in a tone of authority and decision, which repressed the rashness of the most presumptuous innovators. The ancient physicians assigned to each part, and to almost every disease, its appropriate vein. Thus, in affections of the parts below the clavicle, the basilic vein was selected. The cephalic was opened in diseases of the head and face; and the median was chosen when the parts both above and below the clavicle, were involved in the disease.

In like manner, the occipital, the temporal, and the frontal veins, were selected, according as the different parts of the head might be disordered. In diseases of the eyes, peculiar virtue was ascribed to bleeding in the vein between the thumb and the index finger; and that between the little and the ring fingers, was supposed to have some peculiar connexion with the spleen. The ranular vein was opened for the cure of quinsies, and inflammations of the ear; and the saphena, in cases of suppressed menses. In nephritis and stone, they chose a vein on the calf of the leg; and for the cure of barrenness, a small branch behind the ear. But it was impossible that rules, so evidently dictated by caprice, should long be held in veneration. Practice too, necessarily changes, and adapts itself to prevailing theories. So that the places which were selected by the physicians of one age, were denounced by those of the next. But it was not until the discovery of the circulation, that it was thought a matter of indifference from what part blood might be drawn. When physicians became acquainted with the fact, that the whole mass of blood circulated through the system, and that the route was accomplished in the short space of about three minutes and a half;* they began to imagine that no part could be affected by blood-letting, in whatever situation it might be performed, unless by participating with the system in the general consequences of the evacuation. The rules respecting the different situations to be preferred in certain cases, were very soon disregarded; and were observed, so far only as they might comport with the convenience of the operator, or the whim of the patient.

At the present time, blood is seldom abstracted from any part but the arm; and so universal is the impression that the effects of blood-letting are not influenced by the situation in which it is performed, that bleeding in the foot, or on one side in preference to

* There is a difference of opinion on this point among physiologists, as they vary in their computations of the quantity of blood in the body. Some make it not more than 8 lbs. and others estimate it at 100. But allowing the quantity to be 35 lbs. and that two ounces are emitted from the heart of an ordinary man at each pulsation, the time taken up in performing the circulation will be exactly 3 minutes and 44 seconds.

another, is looked upon as indicative of superstition, or of blind attachment to antiquated notions. It cannot be denied that bleeding, even as practised in the present day, is an eminently useful remedy. But is it not a fact, that it sometimes proves injurious in cases where the abstraction of blood is clearly indicated? And could not the same beneficial effects be produced more speedily and effectually by a much smaller loss of blood, were the operation performed in different situations, according to the seat of the disorder? It is, I am convinced, in consequence of overlooking the advantages of this practice, that we find it necessary to abstract such enormous quantities of blood in diseases which, we read with astonishment, were formerly cured by comparatively moderate bleedings; that convalescence is so often a tedious process, protracted far beyond the period in which nature herself can have accomplished a cure, and perfectly re-established health; and that patients may so frequently, with more propriety, be said to have *escaped* from the hands of the physician, than to have been *cured* of their diseases.

It is intended, in the following essay, to take a view of the effects of blood-letting generally; to point out its particular operation when performed upon different parts; and to attempt to shew, that there are certain cases, in which it is proper to abstract blood from certain situations in preference to others.

First, then, of the action of blood-letting in general. On the application of the ligature to a limb, the progress of the blood is retarded in the superficial veins. The consequence is, that they become distended, and contain a greater quantity of blood than they did before. The distention commences at the part to which the ligature is applied, and gradually extends along the canal of these veins. As they become engorged, the resistance to the action of their arteries also is increased. The action of the heart continuing as yet the same, these arterial branches too, become distended with blood; and, finally, this fulness is communicated to the great arterial trunk from which they arise. Besides, as the return of blood is interrupted in these veins, the *vena cava* into which they ultimately terminate, is deprived of that portion of blood which is retained in them; and also of the *vis a tergo* which

they, or their arteries, might supply. So that in the cava also, the return of the blood to the heart, is retarded; and, of necessity, the *internal* veins, which accompany the arteries of the limb, become distended also. But there is another circumstance which contributes to this effect. It has been very satisfactorily proved, that in addition to its propulsive power, the heart performs the office of a suction-pump, by means of the alternate contraction and relaxation of its parietes.* Now it is certain, that this contraction will be in proportion to the stimulus applied to the cavities of the heart; and that the consequent relaxation will be in proportion to this contraction. But we have seen that a considerable quantity of blood is prevented from returning to the heart, by the ligature around the limb. It is evident then, that the suction power of the heart must be diminished; and the inevitable consequence must be, an accumulation in the cava. It will doubtless occur to the reader, that this diminution of the suction power of the heart, ought to affect *both* cavæ equally. But it must be remembered, that the cava connected with the veins of the limb upon which the ligature is applied, has another power operating upon it whereby the return of the blood through it is impeded; and that the two streams of blood poured by the cavæ into the right auricle, are, in some degree, antagonizing forces.

* This doctrine was first advanced by Dr. Andrew Wilson, in a work which he published in London, in the year 1774, and entitled "Inquiry into the moving powers employed in the circulation of the blood;" and it has since been ably illustrated and established by Dr. Carson of Liverpool, who published a work on the same subject, in the year 1815. The idea is now adopted and approved by most physiologists. Indeed, without its admission, it is impossible to account for the circulation itself: for, considering the quantity of blood, the length of its course, and the various obstacles to its progress, it is very improbable that the propulsive force of the heart alone is sufficient; and it is granted on all sides, that the veins are destitute of any very considerable contractile power. Without granting this suction influence of the heart, the use of its valves becomes a mystery; it will be impossible to say why a vein collapses above a ligature which obliterates the continuity of its calibre; or why a punctured vein will not bleed when a ligature is not applied. A very good exposition of this principle, may be seen in a note by Dr. Elliotson to Blumenbach's "Institutions of Physiology." See Warner's Philadelphia edition of 1817, p. 68. I am indebted to his remarks for the substance of this note.

Such being the state of the parts, it is obvious that the limb contains more blood than it did before the application of the bandage; and that the proportion of blood in other parts of the body, must be diminished.* This is what the ancient physicians meant by *derivation*, and *revulsion*. But as they had not the benefit of a knowledge of the circulation, they had no very precise idea of the manner in which derivation or revulsion was produced; and used the former term to denote the afflux of blood to the limb itself, and the latter to express their indistinct notion of its abstraction from other parts of the system. Hence it is, that we find them so inconsistent and contradictory on this subject. But to return. The vein is now opened: but this does not materially affect the condition of the limb. The interruption to the return of the blood continues; and, of course, its consequences remain. So that, although the vein which is opened is less turgid, the other vessels are not evacuated. It is evident then, that the quantity of blood which is abstracted, does not *immediately* affect the state of the limb; but that it contains as much blood, or very nearly so, as it did before the orifice was made. The evacuation then, must be at the expense of the *other* parts of the system; and the limb itself can be affected, only by the general diminution of the circulating mass.

The obvious effects of blood-letting are, then: first, a *diminution* of the general quantity of blood; secondly, an *accumulation* in the part upon which the operation is performed; and, thirdly, a consequent abstraction, or *revulsion*, of it from all the other parts

* I had almost finished this essay, when, in consulting Van Swieten's Commentaries for another purpose, I accidentally met with the following passage, which so entirely coincides with the view I have here taken of the action of the ligature. Boerhaave had, in the aphorism upon which the commentary is written, advised ligatures to be applied to the *legs* in apoplexy: and upon the passage the commentator remarks, "ligaturæ autem artuum tales, ut venas comprimant, arterias non adeo, utpote profundius sitas plerumque, et minus facile comprimendas, efficiunt, ut magna copia humorum colligatur in artubus; cum arteriæ admittant, venæ vero pressæ reditum ad cor impediant; hinc longe minori copia urgebitur caput." Comment. §. 1025. In another passage, he refers to Galen as recommending ligatures (*vincula*) to the lower extremities, in cases of headach. Aph. 396. The efficacy of this practice I have repeatedly experienced.

of the system. But venæsection has another important effect. It slackens, or represses, the *momentum* of the circulation. In whatever manner this effect may be produced; whether it be the result of the diminished quantity of blood, or of any peculiar operation upon the cerebral system; whether it be the consequence of the more tardy circulation of the blood through the lungs, or the effect of all these causes combined; it matters not. The fact has been established by the experiments of Dr. Hales,* and many later physiologists. As this effect of blood-letting does not concern the doctrine which we are about to advance, it would not have been thus particularly noticed, but that it may be adduced in objection to the positions laid down above. It may be contended, that, as the force of the circulation is abated in proportion to the loss of blood; the accumulation which we described as taking place in the arteries, cannot occur. But it must be observed, that this effect of blood-letting is not immediate; that it is not perceptible until the evacuation has been considerable; that it is, frequently, not produced until after the operation; and that it is, sometimes, not observed at all. And further, it could not affect our position, even should the objection be admitted in its fullest extent, and without any exceptions or qualifications. For the same diminution of the *vis a tergo*, which affects the action of the arteries, will proportionably affect the transmission of the blood by the veins.

II. Secondly, we come to apply the doctrine to particular cases. Upon the application of a ligature to the *arm*, the following changes take place in the state of the limb. The superficial veins, the basilic, the cephalic, and the median and its branches, become distended with blood. This plethora extends to their inosculations with the extreme arteries of the limb, and

* The sovereign control which blood-letting exercises over the force of the circulation, is shewn by the following experiment of Dr. Hales. He opened the vessels of living animals, adapted glass tubes to their orifices, and observed to what height the blood rose in the tubes at each systole of the heart: he then drew from the animals different quantities of blood, and observed the force of the heart diminish, in proportion as the blood was abstracted. *Hales' Hæmastaticks.*

finally affects the arteries themselves. The accumulation progresses, until the main trunk of the limb, the brachial artery, becomes unusually full. Nor will it be confined here: it will extend to the axillary and subclavian portions of this trunk, to the arteria innominata, and to every branch that is given off by them.

Now it is obvious, that in proportion as the limb contains an unusual quantity of blood, the other parts of the body will contain less. This is the reason why the pain and dyspnœa of pneumonia, will sometimes be relieved before the orifice is made. And it is upon this principle alone that we can explain the fact noticed by Dr. Sydenham, that although the blood flows ever so fast, yet if it trickles down the arm in bleeding, its abstraction is less serviceable than when it streams horizontally from the vein.* Its not streaming from the orifice, results from the bandage being either too tight, or too loose. In the first case, the influx of blood into the limb, is prevented by the compression of the main artery which supplies it; and, in the second, the derivation can be but small, because the interruption to the return of blood by the superficial veins, is not complete.

From this view of the operation of blood-letting in the arm, it may be concluded, that it may be employed whenever a mere evacuation of blood is required; as in cases of mere plethora, unattended with local congestion, and without any destruction of the balance of the circulation. It is here resorted to as a mere evacuant.

2. It may be employed in all cases where it is desirable to produce a general effect upon the system, without reference to its operation upon any particular part. Thus, it may be used in simple synocha, in the treatment of certain kinds of convulsions, and in those cases in which it is desirable to induce a general relaxation, for the purpose of returning a hernia, or reducing dislocated limbs.

* At vero animadvertendum est, quod si sanguis è secta vena non recto flumine versus horizontem prosiliat, sed per cutem repens perpendiculariter dimanet, utut celeri se proripiat gradu, non æger ab istiusmodi sanguinis missione perinde levatur, ac si modo primum descripto fluat. Sect. VI. cap. iii.

3. It will follow too from the view we have taken, that bleeding from the arm is to be preferred in diseases of the lower extremities, in inflammations of the abdominal and pelvic viscera, in uterine hæmorrhages, in pneumonic affections, and in those cases where the opposite extremity, or the integuments of the opposite side, may be involved in inflammation. It will be observed that I am particular in designating the *integuments* of the opposite side, because I believe it to be immaterial from which side blood is drawn in peripneumony, or pleurisy. This point, however, was, at one time, the subject of very acrimonious contention. While the dogmas of the Arabian physicians prevailed in the schools, it was the universal practice, notwithstanding the authority of Hippocrates and Galen to the contrary, to draw blood, in pleurisy, from the arm of the opposite side. In the year 1514, the celebrated Brissot, with the zeal and pertinacity of a reformer, publicly protested against this practice; and contended, that the operation should be performed on the side affected. He met with great opposition; but, in consequence of the distinguished success which, it is said, attended his practice, he procured many adherents. He shortly after went to Portugal, where he engaged in controversy on this subject with the king's physician. The dispute was referred to the Academy of Salamanca, whose decision procured an edict prohibiting any person from bleeding in pleurisy on the side affected. Charles V. of Spain, was shortly after prevailed upon to issue his royal denunciation against the doctrine of Brissot.*

4. It will appear also, that blood-letting from the arm is not

* The reader may not be aware that there is high authority for giving a preference to one side rather than the other. The learned Van Swieten says, "videtur illa sententia, quæ venæ sectionem in brachio *lateris affecti* suadet, tutissima esse." Dr. Sydenham advises bleeding in pleurisy on the affected side. Sect. vi. chap. 3. Dr. Cullen says, "bleeding will be performed with most advantage in the arm of the side most affected." First Lines, § 362. And Dr. Wilson Philip, though he speaks with great indifference, seems to think that the preference is not altogether groundless. Wilson on Fevers, vol. ii. p. 192. Notwithstanding, I have never observed any difference of effect in this case; nor can I conceive any reason why there should be any.

advisable in congestions and inflammations of the brain; because the arteries by which it is supplied, necessarily participate in the derivation which is effected. The older surgeons strenuously prohibited blood-letting in the *right* arm in these cases, while they allowed that it might occasionally be advantageous when performed in the left. This remarkable precept will not appear so inconsistent and absurd as it has been thought, when we consider that the left carotid artery arises immediately from the aorta, and consequently undergoes some degree of revulsion when the operation is performed on the left arm; but that, as the right carotid and subclavian arise from a common trunk, and the vertebral artery is given off by the latter, they must all be involved in the derivation produced in the extremity of that side. They had, no doubt, observed a difference in the effects of bleeding in these cases; and, although unable to account for it, were led by experience to regulate their practice accordingly. Since taking this view of the subject, I have repeatedly endeavoured to observe this difference of effect in the operation of blood-letting; and it is a matter of uniform experience with me, that fainting is induced by bleeding in the left arm much more readily than in the right. The rationale of this fact is easy. A certain degree of tension is indispensable to preserve the energy of the brain; and a constant communication of this energy, is necessary to maintain the vigour of the heart's action. Hence nature has contrived a peculiar conformation of that organ, thereby to retard the passage of the blood through its vessels; and hence profuse hæmorrhages, or artificial evacuations of blood, by taking off that fulness and tension, occasion syncope.* Nothing is more common in the operation of phlebotomy; and it can generally be prevented in these cases, by a recumbent posture. It is evidently by diminishing this necessary tension of the brain, that tapping and delivery induce deliquium. In these instances, a change takes place in the distribution of the blood; so that a larger portion of it is derived into other parts of the sanguiferous system, and there occurs a propor-

* See Cullen's First Lines, Vol. II. § 1170, for an ingenious exposition of this theory of syncope

tionate abstraction from the brain. It is thus also, that we are to explain the syncope consequent upon bleeding. On the removal of the ligature, all obstruction to the free course of the circulation in the limb, is removed. The consequence is, that an instantaneous afflux of blood takes place to that part; so that the tension of the brain is suddenly affected, and that organ necessarily remains in some degree of collapse, until the equilibrium of the circulation is restored.* Now, it is obvious, from the view which has been given of the distribution of the arteries that supply the brain, that the quantity of blood in that organ, will be more readily diminished by abstracting it from the left arm, than by bleeding in the right.

5. It is evident that bleeding from the arm of the side affected, is improper in cases of rheumatism of the intercostal muscles. We are so much accustomed to draw blood from the arm, without any reference to the seat of the disease; and, I may add, we are so much in the habit of using this remedy empirically, and without reflecting upon its operation, that this remark may, to many, appear frivolous. But it will not seem so unimportant when it is considered, that the parts in which this affection is seated are supplied with blood by the thoracic or external mammary arteries, which arise from the axillary trunk; and that the vena azygos, which receives the blood of the intercostal veins, terminates in the superior cava. It is because this circumstance is not adverted to, that it has become almost proverbial, that this disease is not so much benefited by blood-letting as might be expected from its pathology.

6. It is obvious, in short, that blood-letting in the arm is contra-indicated, whenever parts are involved which receive their blood from branches of the subclavian, axillary, or brachial arteries.

But it may be said, that blood-letting in the arm is continually

* ... is true, that the blood is returning to the heart with equal rapidity; but ... arrive at the brain, it must perform the circuit of the pulmonary ... I do not remember to have ever seen this remarkable fact ex... this is the only explication of it that I can devise. I believe it ... y in which it can be accounted for.

resorted to in all those cases in which, agreeably to our theory, it is contra-indicated; and that, notwithstanding, it is frequently attended with signal advantage. This cannot be denied. But if it proves serviceable in these circumstances, it can be only by the evacuation it procures; so that the relief it affords can be neither so complete nor so speedy as it might be, were the operation performed in a more suitable part, and the full benefits of revulsion procured. It would no more be proper to condemn, on this account, the principles advanced, than it would be logical to discard the general rule, that blood-letting is to be performed only in diseases of a sthenic diathesis, because it sometimes proves useful in diseases of an opposite character.

III. In treating of bleeding in the *foot*, it will be sufficient to refer to our remarks on the effects of blood-letting in general; pointing out, however, the peculiarities of this operation. The effects of compression by the ligature, will here be the same as we have seen to take place in the former instance; with the exception, that while blood-letting in the arm is revulsive with respect to the opposite limb,* blood-letting in the foot produces derivation in both of the lower extremities. For, as the iliac arteries proceed from the same trunk, diverge at the same angle, and are of the same calibre; and as the iliac veins unite to form the inferior cava; the derivation which is produced in one extremity, must necessarily affect the other in nearly the same degree.

It will be remarked, too, that the derivation in this case is more extensive than in the former; as, from the anatomical connexions of the parts, the pelvic and abdominal viscera necessarily participate in the derivation which is produced.

Further, as another peculiarity of bleeding in the foot, it will be kept in view that the extremity is immersed in warm water.

* Any one may convince himself of this by a very simple experiment. Apply a bandage to the arm, as in bleeding; and the veins of the other arm will be observed to lessen very remarkably. Upon removing the ligature, the increased flow of blood into this arm will be very sensible. Again, immerse one leg in warm water, and the veins of the opposite extremity will become turgid.

The pediluvium was, no doubt, had recourse to for the purpose of rendering the vein which was to be opened, more perceptible. But this is not the only effect it produces. It is evident that it must greatly add to the derivation created by the ligature, by facilitating the distention of the parietes of the veins, and thus inviting a copious afflux of blood to the limb. This effect of the pediluvium is too remarkable to have escaped the observation of physicians; and, accordingly, we find that they have always availed themselves of its advantages in the treatment of diseases. It would be easy to adduce a long list of ancient authors, who laud the virtues of the pediluvium in the language of enthusiasm. Nor has it been less esteemed by modern physicians. Van Swieten informs us, that he had seen an epidemic pleurisy, in which there was a great tendency to the brain; and that he averted this dangerous metastasis by the early use of the warm bath to the feet.* The celebrated Dr. Whytt, in his invaluable work on nervous disorders, relates several interesting cases in proof of the excellence of warm pediluvia in fevers attended with *delirium;* and declares, that "he had saved more patients who appeared to be in great danger, in the delirious state of fever, by this means, than by any other remedy."† It is recommended in phrenitis and other affections of the head, by almost every practical author, both ancient and modern; and its efficacy in restoring the menses and recalling suppressed lochia, is a matter of popular notoriety.

It is unnecessary to extend our remarks on the action of warm pediluvia, as it is sufficiently manifest from the view we have taken of their operation, and of their employment in diseases, that they exert a powerful impression upon the system; and that their beneficial effects are wholly ascribable to the derivation and revulsion they procure. It must be obvious, then, that, independently of every other consideration which can give it an advan-

* Memini me quandoque talem pleuritidis epidemicæ speciem vidisse, cui morbo, nisi ab initio jam balneis pedum, &c. prospiceretur, familiaris erat in phrenitidem primis etiam diebus mutatio. Comment. in § 772.

† Observations on the nature, causes, and cure of those disorders which have commonly been called *Nervous, Hypochondriac,* or *Hysteric,* &c. &c. Edinburgh, 1775. 8vo. p. 446—458.

tage over blood-letting in the arm, bleeding in the foot must, in many cases, be the preferable practice.

1. I conclude, that it is to be preferred in apoplexy, in phrenitis, and in all other cases where it is desirable to relieve the brain by the detraction of blood.*

2. It is to be preferred in affections of the upper extremities, in diseases of the throat, in hæmorrhages from the nose,† &c.

3. It is to be preferred in suppressions of the menses.

Whatever differences of opinion there may be respecting the nature and the proximate cause of the menstrual evacuation, there is but one indication for restoring it when suppressed; and that is, to excite the action of the uterine vessels. This indication may be fulfilled, by imparting to them a certain degree of tone; or by inducing a local plethora in the organ, which will be adequate to overcome the constriction of their exhalent mouths. I would not be understood to convey the idea, that this accumulation does, by its *mechanical* distention, break down the obstructions that may exist in the uterus, or force a passage for the blood by the rupture of the vessels: I believe, however, that a certain degree of fulness and tension, is necessary to excite in the organ that action whereby the menstrual function is performed. And, accordingly, all the emmenagogues that are usually prescribed with the vague idea of their specific virtue in propelling the catamenia by some *peculiar* action upon the womb, are conducive to this end in proportion to the derivation which they create in that organ. Perhaps I would not expose myself to contradiction, were I to declare, that not a single article of this class has any immediate ac-

* In apoplexy, Boerhaave recommends ligatures to the *lower extremities.* Aph. 1025. In phrenitis, he says, "venæ sectio instituenda larga—*in pede,* &c.—*pediluvia* applicanda," &c. Aph. 781. And Van Swieten explains the operation of the baths thus: "his enim laxantur vasa partium inferiorum. Versus hæc loca major copia humorum ducitur, adeoque a superioribus revulsio fit." Comment. in Aph. 781. Dr. Rotheram, in his note to Dr. Cullen's chapter on Phrenitis, advises "large bleedings on the *lower extremities.*" Cullen's First Lines, § 295. Chap. IV. Dr. Lind recommends blood-letting in the *foot* in the commencement of some tropical fevers, in which there is pain in the head. On Hot Climates, p. 178.

† In epistaxis, immersing the feet in warm water will arrest the hæmorrhage, when plugs, and ice, and bleeding from the arm, have totally failed.

tion upon the womb itself;* and that this organ is affected only by sympathising in the action which they excite, or by participating in the derivation which they procure, in the neighbouring parts. If we examine the operation of any of the principal emmenagogues; whether it be calomel, aloes, scammony, or any other drastic; whether it be savin, cantharides, or hellebore; we shall find that its principal and immediate action is confined to some neighbouring organ, and that it determines an afflux of blood to that part. It is exactly upon this principle that Corvisart† found leeches to the pudenda so serviceable in provoking the menses, and that Dr. Chapman‡ has applied blisters to the thighs with such signal success.§ It was with the view of producing a temporary plethora of the uterus, that Dr. Hamilton proposed compressing the crural arteries by a tourniquet. Had he applied it so as merely to compress the veins, he would more frequently have succeeded. In the experiments of Dr. Home, it seemed evidently to determine to the head.‖

But to return to the subject of bleeding in the foot. We have seen that the effect of the ligature to the lower extremity, is to produce a plethora in the iliac arteries; and we know that the womb is supplied with blood principally by branches of the hypogastric arteries, which arise from the external iliacs. It is evident, then, that the menstrual discharge will be most readily restored by letting blood from the foot. Nor is this operation entitled to a mere preference. Bleeding in the arm, I believe to be *positively injurious* in cases of suppression; as, by the revulsion it produces in the uterus, it diminishes the necessary plethora in that organ, and represses that action of its vessels which is indispensable to

* I will except the *rubia tinctorum*, and the *ergot*, as they have no sensible operation, except what they exhibit upon the womb. But, notwithstanding the encomiums of Dr. Home, the former article has fallen into disrepute; and the emmenagogue virtue of spurred rye is yet a subject of doubt.

† On the Heart, &c.

‡ Theurapeutics and Mat. Med. vol. ii. p. 54.

§ It was formerly a fashionable practice to apply cups to the thighs. Astruc, Maladies des Femmes. Tom. i. p. 195. Friend, Emmenologia. Opera, Vol. I. p. 126.

‖ Home's Clinical Expts. and Hist. p. 411.

restore the suspended function. It would neither be consistent with truth, nor with policy, to declare, that blood-letting in the arm should be prohibited in *every* case of amenorrhœa. This practice is pursued continually, and often with the most decided success. But this is no objection to the principles I have advanced, or the precepts deduced from them. There are, undoubtedly, instances of suppression, in which bleeding from the arm is not only admissible, but absolutely necessary. In those cases in which the retention of the menses would seem to be owing to an inordinate engorgement of the uterine vessels; in which there is superadded an inflammatory disposition, or painful tension of the womb; letting blood from the foot would certainly be pernicious, and bleeding in the arm would be indispensable. But with these exceptions, I hold it as a principle established by the soundest pathology, that bleeding in the *foot only* is proper in suppressions of the catamenia.*

4. Blood-letting in the foot is to be practised in suppressions of the *lochia*. It is unnecessary to dilate here, as the reasons of this preference must be obvious from what has been said respecting the practice in amenorrhœa. It is proper to observe, however, that should suppressed lochia be complicated with an inflammation of the womb, bleeding in the arm is to be preferred.

5. Blood-letting in the foot is contra-indicated in diseases of the lower extremities, in inflammatory affections of the pelvic and abdominal viscera, in uterine hæmorrhages, in hæmorrhoids, &c. It is necessary to remark, that this operation is not to be excluded entirely in inflammations of the uterus; for when the menses, or the lochia continue in some degree, and there is any probability of restoring the evacuation, it may be resorted to with propriety and advantage.†

IV. Bleeding in the *jugular vein* is not unfrequently practised. It is evident, however, that the compression employed, must produce an accumulation of blood in the brain. This operation can seldom be necessary, as, except in young children and persons of

* Home's Clinical Experiments, Histories, &c. p. 419.

† Astruc, Traité des Maladies des Femmes. Tom. iii. p. 56. Paris 1761.

a very corpulent habit, sufficient blood can always be procured by bleeding in other situations. Yet, as so large a quantity can be abstracted in a short time from this vessel; and, in consequence of the orifice being situated between the bandage and the part affected, a direct detraction of blood is procured; it may doubtless be practised with advantage in many cases of engorgement of the brain. Perhaps it should be preferred, when it is necessary to obtain very speedy relief. But, I believe, it should seldom take the precedence of bleeding in the foot; as, in the latter operation, in addition to the evacuation, the immense advantage of revulsion is gained.

Arteriotomy, or opening the temporal artery, I believe to be perfectly useless. It does indeed afford the benefits of an evacuation of blood: but these may be obtained more conveniently, more speedily, and more effectually, by bleeding in the arm or foot.

Having completed my remarks on the subject of blood-letting, I commit them to the judgment of the profession, with a firm assurance of the truth of my statements, and, I confess, with some confidence in the correctness of my reasoning. Yet I feel a hesitancy in advancing opinions, and in recommending modes of practice, so contrary to those which are usually received; sensible of the difficulty of restoring confidence in what is discarded, or of procuring it for what is new. "*Res ardua vetustis novitatem dare, novis auctoritatem.*" Plin.

Observations on Cataract, and the various Modes of operating for its Cure. By G. Frick, M. D. of Baltimore.

The errors which for a long time prevailed, respecting the true nature of cataract, are to be ascribed altogether, to that neglect of anatomical and physiological research, which equally obscured many other subjects of pathology; and they were more liable to be perpetuated in the present case, from the particular office allotted to the crystalline lens, the seat of the affection now under consideration. Supposing this to have been the immediate seat of

vision, it was difficult to understand why the destruction of this faculty followed any accidental opacity of the lens, or how the removal of this body, from its natural position, should again restore the visual power. The Academy of Sciences of Paris contested the subject for a length of time; but were unwilling to yield up the point, notwithstanding the number of facts which concurred daily to shake their faith. The very persons who opposed the doctrine, doubted the observations they brought forward to substantiate their own. The slow progress of the cataract, the plain surface which the crystalline lens presented, the facility with which it was removed by the needle, were so many proofs, fully corroborative of the conjectures they had formed relative to its nature: supposing it nothing more than the solidifiable parts of the aqueous humour, precipitated and concreted in a membranous pellicle around the lens, and thus destroying its functions. A circumstance which added the greater plausibility to these conjectures, was, their mistaking the capsule of the lens in the membranous cataract, for this very pellicle. It was observed, that in persons who had been operated upon for cataract, in the ordinary mode of depression then practised, the transparent crystalline occupied its natural position, while the membranous capsule was opaque, plaited, and plunged a certain distance into the body of the vitreous humour.

From the more accurate knowledge we possess, at present, relative to the true seat of cataract, connected with other points in its pathology, we are able to direct our efforts more rationally, as well as to act with more precision and satisfaction; but still there are some circumstances respecting the disease, not sufficiently or generally understood. Some remains of the old doctrine continue to influence our opinions and practice; much is taken for granted, which is by no means true, and practical inferences are drawn from tenets not at all admissible. It is with a view to correct some of these errors, as well as to exhibit what is known and taught by the principal oculists on the continent of Europe, relative to this disease, that the writer has thought fit to draw up the following observations.

As long as surgeons continued to dispute about the merits of

couching and extraction, without any reference to the particular cases in which they were applicable, it was of little importance to attend to the varieties in the condition of the cataract itself, as the supporters of the one or other method of operating, couched, or extracted indiscriminately in all. Since the new mode of puncturing the capsule, and permitting the lens to escape into the aqueous humour, has come into vogue, it has become too much the fashion to neglect the older methods; but a little experience and observation will shew, that the practice, though very efficient in some, is by no means applicable to many, and that not the smallest number of cases which fall under our notice. The better, therefore, to point out those, in which these several modes are rendered necessary, we must be allowed to dwell more particularly on the different varieties of Cataract, whilst we endeavour, by a little attention to the phenomena, which each presents, to establish a just diagnosis.

The English writers, on this disease, admit but of four species; the continental authors have added several others. It will be sufficient, perhaps, for our purpose, to adopt the English classification, not forgetting, however, to notice their various complications.

Mr. Pott* already observed certain circumstances in the appearance of the cataract, which led him to judge, before the operation, of its consistency. This was, indeed, doubted afterwards, by Mr. Hey; and although later writers have continued to speak of the several varieties, none of them have ventured to lay down any precise rules, to guide our judgment in forming a true and proper diagnosis. If my readers should deem me a little prolix in this part of my subject, its importance alone must plead my only excuse. I begin with the

Cataracta dura, or *Hard.* Mr. Pott has very justly observed, that in this variety of cataract, the lens is smaller than in its natural state; hence, the patient is enabled to see large objects tolerably well, particularly in the morning and evening twilight, when the absence of the strong light of day permits the pupil to expand. The same dilatation may be brought about artificially, by the ap-

* Vid. Chirurg. Works, Edit. 1808. Vol. III. p. 147.

plication of belladonna. The patient produces the same effect, by overshadowing the eye with his hand, when he wishes to view any object more distinctly. The iris, being perfectly free from the pressure exerted, when the cataract is of a larger size, preserves its usual irritability. The posterior chamber is enlarged. The lens is of a dull whitish, or horn colour; sometimes of a yellow reddish tinge, which causes all objects seen by the patient, to appear of the same hue. The centre, or nucleus, is generally deeper coloured, as well as more firm, than the circumference. We may lay it down as a general rule, that all cataracts occurring at an advanced period of life, without previous disease, are hard. There are, no doubt, some exceptions to this rule; and as it is not uncommon to meet with the perfectly hard cataract, in children, there are not wanting instances of its being found fluid in old persons. If the opacity be seated purely in the lens, its progress is very slow; and as the disease becomes more advanced, a black ring, which is nothing more than the light of the area reflected from the darkened capsule, will be seen to surround the internal margin of the iris. In such persons as have the iris of a lighter or greyish colour, this ring is of course less perceptible. This variety of cataract may exist a great number of years, without the capsule becoming in the slightest degree affected.

II. *Cataracta Caseosa seu Gelatinosa.* We have seen in the cataracta dura, that the lens is shrunk and diminished in size. In this species it is, on the contrary, much enlarged; hence the iris is pressed forward nearly in conjunction with the cornea, and the anterior chamber of the eye is almost destroyed. For the same reason, the iris is rendered sluggish in its motions, and is commonly thickened too in substance. The patient's vision is nearly or completely obscured; nor is it at all improved in the twilight, nor by the application of belladonna, or other narcotic substances, which have the effect of dilating the pupil.

In this variety of cataract, the two surfaces only of the lens may be rendered caseous, whilst the centre or nucleus preserves its usual consistency, or both may be similarly affected; the first most commonly happens. The opacity is never uniform; and the lens appears always more or less spotted or flocculent. If the nucleus

be solid, the cataract is generally stellated. The black ring encircling the lens, which we noticed in the first species, is wanting in this variety.

III. *Cataracta Lactea seu Morgagniana.* The lens, in this variety, is unequally clouded; darker in some places, and more transparent in others. These darker coloured spots or flakes may be observed to shift their position and alter their form, by rubbing the eye, or moving it about rapidly in the orbit. If the eye be kept any time at rest, there may be observed two very distinct layers of fluid; the upper of a much brighter colour than the lower. These instantly mix again and vanish, by shaking the eye or head violently. The colour is then a pale white, approaching to that of skimmed or thin milk; hence, it has been denominated lactea. The iris is sluggish, and oftentimes driven forward, so as to present a convex surface. The congenital cataract is generally, though not always, of this kind.

Professor Beer supposes the C. lactea to be caused, in many instances, by the action of certain gaseous substances upon the eye, and particularly by those gases evolved by the action of acids in the oxydation of metals. In his lectures, he relates an instance of an elderly woman, the wife of a goldsmith, in whom a cataract formed in the short space of a few hours; and he was obliged, from the importunity of the patient, to operate the succeeding day. As soon as the capsule was opened, a quantity of milky gelatinous matter issued out: the lens, which was perfectly transparent, was extracted by means of a needle, and a very slight pressure exerted upon the eye-ball.

IV. *Cataracta Capsularis seu Membranacea.* This opacity, as the name implies, is seated in the capsule surrounding the lens. The anterior or posterior capsule, or both, may be obscured.* In the latter case, the vision is completely destroyed, nor is it much improved by the application of narcotic substances. The opacity seldom commences in the centre of the pupil, but more generally from its margin, in the form of small shining spots or stripes; and hence it is, that even in the most mature state of the cataract, the

* It is easy to understand how the one surface may be affected without the other, from the different sources whence they receive their nourishment.

opacity is never uniform. It seldom exists any length of time, before the lens becomes affected. If the anterior capsule be alone opaque, the cataract will be seen much more convex than usual: this membrane, being at the same time much thickened, lies close behind, or even in contact with the iris, so as to render the latter almost motionless. If the posterior be the seat of affection, the cataract will be seen lying deep within the eye, presenting a concave surface. As this species of cataract is frequently the result of inflammation of the neighbouring parts, propagated to the capsule, we find this membrane oftentimes not united to the lens only, but strongly adhering to the membrana hyaloidea posteriorly.

A more important distinction, and one which it becomes every surgeon to attend to, before forming his prognosis, is the state of simplicity or complication of the cataract. We term the disease simple, when it is accompanied with no other morbid alteration of the organ of vision, which can affect its functions, or no complaint of the general system; and complicated, when there is superadded to the opacity of the lens or its capsule, some other local or general affection. These two states of the disease have, unfortunately, been too much neglected by all practical oculists; who, in general, are content to submit their patients to the operation, provided there is no other visible derangement of structure or disease about the eye, or there is no sense of pain or tension in the head; and if, perchance, they discover disease in any other part of the system, the connexion between this and the cataract is supposed to be so remote, as not to influence at all the success of their operations.

The following case, related by Professor Beer, in his lectures, fully illustrates the importance of attending to this point in practice. He was called upon to operate for cataract in a man, whom he supposed to be perfectly healthy, but who was affected, entirely without his knowledge, with ulcers upon the feet. The professor had scarcely reached home, after completing the operation, when he was sent for in great haste; and on returning, he found his patient in the greatest pain with his eye, and the upper lid and conjunctiva excessively swollen. He then learnt, for the first time, of the existence of these ulcers, and that they had dried up, immediately after the operation. Sinapisms and warm cataplasms

were ordered, to bring back the discharge; and the patient was almost as suddenly relieved.

It would be impossible to notice all the varieties of complication, which can thus occur. It will be sufficient perhaps, to enumerate merely the most important.

Cataracta Acreta. This is always the result of inflammation in the capsule and neighbouring parts; during which, a quantity of coagulable lymph is thrown out, so as to unite the anterior capsule with the iris, or the posterior with the membrana hyaloidea. When this latter circumstance took place, (which can only be known after the operation,) the cataract was termed by the old writers upon the disease, elastica, from its constantly rising again after the needle, when they attempted to depress it. The union with the iris may be easily recognised by the angular appearance of the pupil, or by dilating this with the belladonna. Most commonly, the inner membranes of the eye have suffered so much from the same inflammation, that the patient is unable to distinguish light from darkness; and in this state of the disease, any operation must, of course, prove fruitless.

Cataract is sometimes complicated with glaucoma; and this is readily distinguished by its peculiar sea-green colour. The lens is enormously enlarged, projecting without the pupil into the anterior chamber; the iris is sluggish; the pupil very much dilated, and drawn, as it were, towards the inner canthus; the patient is completely blind; the whole eye is varicose, and very hard to the feel. The disease is accompanied throughout its progress, with violent and incessant pain of the head.

Amaurosis is not unfrequently combined with cataract. A widely dilated pupil, and immovable iris, were at one time looked upon as infallible criteria of this complication; but we have already seen that these symptoms are often caused by nothing more than the mechanical pressure exerted by the lens, and they are by no means constant and necessary symptoms of amaurosis. If, however, there be connected with these appearances of the iris, evident atrophy of the eye; or if the patient be unable any longer to distinguish light from darkness,* we may pronounce with certainty,

* The surgeon should be cautioned, however, never to rely in any instance,

there is amaurosis. The diagnosis is more difficult, where the latter commences, when the cataract is already mature, and the difficulty is increased still further, where the patient is unable to give a distinct account of the origin and progress of his complaint. Pain in the orbit or neighbouring parts, should always excite our suspicion, as cataract is generally unaccompanied with any particular pain. The same may be said of the sensation of tension or pressure, which the patient sometimes experiences towards the root of the nose, or in the supra-orbitary ridge.

Other local complications with cataract, are much more readily distinguished. Such are, staphyloma, hydrophthalmia, leucoma, ulcers of the cornea, pterygium, psorophthalmia, and the various species of inflammation.

Cataract may be complicated with some more general affection. It is not unfrequently associated with gout, scrofula, syphilis, herpes, &c. and in some of these cases, we may very much retard, and often arrest the disease in the lens, by the usual remedies for these complaints.

The proximate cause of cataract is still involved in much obscurity. In general, we may consider it as the result of inflammation. I am well aware, that in advancing this opinion, I shall have to contend with many who deny that the lens is possessed of vitality, and therefore incapable of taking on any inflammatory action; and though no direct proof can be adduced of the vascularity of this body, phenomena, inexplicable upon any other supposition, are sufficient to confirm our assent to the latter doctrine. If the lens be nourished by simple imbibition, how, I would ask, shall we account for the development and growth, for the changes it undergoes in colour and consistency in the fœtus, about the time of birth,* not to mention the alterations of structure it suffers

for this information, upon the simple ipse dixit of his patient, but to examine most scrupulously for himself. Ask a person labouring under blindness from cataract, or any other cause, whether he has any sensation of light, and he will answer most unequivocally, "No! I am stone-blind." Yet direct this same individual to fix his eye upon the window, the fire, or any luminous object, and intercept the rays of light, by moving the hand, or any opaque body between the eye and object, and he can distinctly tell you.

* "There are very good grounds to suppose that the lens in the embryo

under disease. It is true, that comparative anatomy, which has thrown so much light on many other points connected with the physiology of the human body, has not, as yet, furnished any unequivocal fact, respecting the mode in which the lens is supplied with nutriment; but it is equally undeniable, that it has not afforded, in the extensive range of the animal creation, another instance of similar nutrition. Why, then, should we grant an exception, so extraordinary, to the lens? The lamellated and fibrous* structure of this body, its different degrees of consistency, as you approach its centre, or recede towards the circumference,—are these not sufficient proofs that it is nourished, like all other parts of the body, by blood-vessels? The central artery of the retina, has been frequently traced to the posterior capsule, where it forms a beautiful and complicated net-work, stretching even to the anterior surface, and anastomosing with those coming from the ciliary arteries; yet, shall we believe these vessels to terminate here, because we are unable to pursue them any further? Zinn, whose labours as an anatomist, are well known, and have only been equalled, perhaps, by the splendid researches of Sœmmering, thinks he has seen, in one of Lieberkuhn's most successful injections, a vascular connection between the lens and capsule; and this opinion is now advocated by the celebrated Professor Beer, of Vienna.†

The diseased state of the lens offers, perhaps, still more decisive proofs in favor of this doctrine. Several cases are on record, where this substance has undergone complete ossification. Morand‡ describes a strong cataract, accompanied with ossification of the vitreous humour. I have seen an ossified lens of this kind in Professor Beer's cabinet, at Vienna. Instances are not rare, of suppuration of the lens: hence, the denomination of cataracta purulenta, and that variety which Adam Schmidt was the first to

state, is opaque and fluid; and that it is only as the fœtus approaches to its full growth, that it becomes transparent, and of greater consistency." V. Walther's Abhandlungen.

* Vid. Sœmmering.

† Barth, the preceptor of Beer, and one of the first oculists of his time, is said to have undertaken a journey from Vienna to Berlin, for the express purpose of seeing a preparation of this kind in the Waltherian cabinet.

‡ Vid. Histoire de l'Academ. Roy. des Sciences, 1730.

describe,—cataracta bursata, seu cum bursa ichorem continente.* The fluid of the cataracta lactea, approaches sometimes so nearly to the nature of pus, that at one time it was termed the purulenta. The whole lens is not necessarily destroyed in these cases; but some part of it is oft remaining, just as suppuration may take place, partially in the liver or any other organ of the body, without destroying the whole structure. Professor Walther has very aptly termed this species of cataract, a "phthisis of the lens."

The disappearance or absorption of the lens in children, can be explained upon no other principle than that which we are advocating.† How else can this be effected, than by the natural process we witness in other structures of the body; and if there are lymphatics in the lens, why should there not be blood-vessels also? The fact of this absorption is too well confirmed by the experience of many oculists, both in England and on the continent, to admit of any doubt. If a child have suffered any violent contusion of the eye, or if any sharp instrument have penetrated the cornea, and touched the lens, cataract will be the inevitable result. In a short time, this opacity will no longer present the rounded appearance it did in commencement; but a flat surface will be seen behind the iris, and, on examination after death, on attempting to remove this with the needle, there will be found an opaque, tough, almost leather-like membrane, which is nothing more than the thickened capsule, with its anterior and posterior surfaces firmly united. The same effect is caused by the frequent convulsion of the muscles of the eye in children, or even by the action of too intense a light on

* The existence of this species of cataract has, indeed, been denied by many very celebrated oculists; the fact, however, cannot be disputed, as Professor Beer has met with several instances of a similar kind. The capsule, in these cases, contains, besides the lens, a small sac or membrane, filled with a thin ichorous matter. It is generally situated behind the lens, so that it is necessary, in operating, to extract this first. In one instance which occurred to B. this bag was left behind in the anterior chamber, but in a short time, was completely absorbed.

† Mr. Demours relates the case of a man who, having been afflicted fourteen years with cataract in both eyes, discovered, one morning, after passing a feverish and restless night, that his sight was much improved; and from this time forward, his vision grew gradually better, until he was completely restored. Vid. Traité des Maladies des yeux, 1818.

the more delicate and susceptible eyes of infants, shortly after birth; and, hence it is, that this species is often confounded with the congenital cataract. Schmidt has denominated it the cataracta arida siliquata. Professor Walther,* of Landshut, after describing very minutely the appearance of the capsule under inflammation, as seen through a powerful microscope, affirms to have discovered likewise, the more delicate ramifications of vessels in the body of the lens itself. These he supposes, however, to be merely the effect of disease, and not to exist, at all, in the natural or healthy state of this body. As the capsule or uvea, in an inflamed condition, very frequently shoot forth vessels from one to the other, so he thinks the former may even extend its ramifications into the lens. Although this explanation may very well serve to suit the doctrine of imbibition, which the professor advocates in the commencement of his paper, it is more rational to believe these vessels existed prior to all disease, and were only rendered visible in the inflamed state, from their admitting the red particles of the blood, instead of the transparent fluid they carried in their natural condition. As this gentleman has been the first to throw any light on the true pathology of cataract, and has most satisfactorily proved its origin in general from inflammation, it is a little surprising he should have adopted the doctrine of imbibition, when his whole reasoning is so much better reconciled with that of nutrition by means of vessels. Hence, too, he is led to suppose, that inflammation of the lens is always preceded by inflammation of the capsule.† This is, no doubt, in many instances, very true, and capsular cataract terminates very generally in opacity of the lens; but in a great majority of cases, we see pure lenticular cataract, without any affection at all of the capsule. Are we then to suppose, that inflammation may take place in the capsule sufficient to excite cataract of the lens, without causing opacity in this membrane itself? The rapidity with which cataract sometimes forms, is a sufficient objection to the doctrine advanced by the professor; unless we can suppose as sudden a creation of new vessels. Instances

*Vid. Abhandlungen aus dem Gebiete der praktischen Medicin. Landshut, 1810.

† Ibid. p. 64.

are on record, where the lens has become opaque in a few hours; thus, Mr. Hey* relates the case of a lady, who became blind immediately after parturition. Supposing it then to be granted, that the lens is nourished, like all other parts, by blood-vessels, there can be no difficulty in believing them subject to the same diseased actions, and that this body is just as competent to take on inflammation, as any other organ or structure of the whole frame. A number of phenomena, which accompany the first formation of cataract, tend to prove that a slow, chronic kind of inflammation is, at the same time, going on in the deeper seated parts of the eye. Hence, we may account for that painful susceptibility to light, which affects all persons, more or less, in the commencement of this disease. Scarpa already takes notice of the greater convexity of the lens in the first development of cataract; and shall we not attribute this to the swelling or increased volume, which necessarily results from inflammatory action?

It is a fact well known to the best practical oculists, that inflammation is much more liable to succeed to the operation for cataract, when this is undertaken in a recent stage of the disease, than where it is delayed to a remoter period; and this may be one reason, perhaps, why the ancients operated only in what they termed the ripe state of the disease. This observation fully agrees with what we witness in other structures of the body, under a state of inflammation. There is scarcely a single disease which is not capable, by metastasis, of producing cataract. We have instances on record, where it has been caused by repelled itch, suppressed hæmorrhoids, menses or blennorrhagia; from gout, syphilis, &c. How it is that such metastasis takes place; whether, as the ancients supposed, some morbific matter is removed from one part to the other, or it is owing merely to a certain consensus of action between the different parts of the body, we are unable to explain: so much we know, that whenever this change takes place, the same sort of action ensues in the organ secondarily affected, so far as this is compatible with its structure, as occurred in that primarily diseased. Thus we see erysipelas of the face, or the erup-

* Vid. Surgical Observations, p. 39.

tion in scarlet fever, suddenly transferred to the brain, and inflammation of its membranes is the consequence. I do not say, that these metastases are always purely inflammatory; something specific is, no doubt, superadded in many cases, to the inflammation. May this not account sufficiently for the varieties we witness in cataract?

The consequence of inflammation in a transparent part, we know to be, opacity: thus, inflammation attacking the cornea or conjunctiva produces specks, or the membrana hyaloidea of the vitreous humour, causes glaucoma; and a variety of facts tend to prove, that the same observation holds good also with respect to the lens. A person is struck a violent blow upon the eye, or the lens is wounded but very slightly by the point of some sharp instrument, which has penetrated the cornea. If the person be young, we have already said, the lens may be absorbed; if he be more advanced in years, the lens will retain its natural position; but, instead of preserving its usual transparency, becomes gradually opaque, and finally destroys completely the function of vision. This change sometimes takes place more rapidly, even before the symptomatic inflammation in all the other parts of the eye has subsided. Another is attacked with violent iritis; the disease spreads to the retina, capsule, and even to the lens itself; the inflammation abates; but there succeeds to this, closure of the pupil, and opacity of the lens. I would not wish to be understood, however, as saying, that this is the most common or frequent termination of iritis; such disorganization of structure is, on the contrary, comparatively rare, and can succeed to the most violent cases of disease only, or where this has been neglected or improperly treated in the commencement.

Arthritis, small-pox, measles, &c. have been assigned as predisposing causes of cataract; and how otherwise shall we account for their operation, than by a state of inflammation which they induce?

I do not pretend to assert, that cataract is, in every instance, preceded by inflammation: thus, that species which occurs in very advanced age, may be ascribed rather to the rigidity or want of tone in the vessels of the lens, which prevents the free circula-

tion of fluids through this body, or causes them to be totally obstructed. Hence it is, that this cataract differs materially in several circumstances from all others. It is peculiarly hard; the lamellæ are no longer separable, as in the healthy lens; but are firmly united, and there is not the least appearance of that subtile halitus, which enveloped these lamellæ in the healthy subject. The same effect we often see produced in the cornea of old persons, causing that appearance which is denominated arcus senilis.

We may except, likewise, from those cases of cataract produced by inflammation, such as are caused by the too intense and long continued application of light to the eyes, or by the excessive exercise of the eye itself. Whether the lens may be supposed to suffer a premature decrepitude in these instances, or whether the light acts, as Mr. Richerand* supposes, by oxydating this body, is a question not easily determined. The fact is, nevertheless, true: thus, cataract is very common among men of studious habits; artizans who are accustomed to work much with the microscope; bakers, cooks, smiths, &c. Petit has observed, that of three hundred patients afflicted with cataract, three-fourths of them were persons accustomed to work in the sun, in a stooping posture, whilst their eyes were expanded to the vivid light reflected from the soil. It is very probable, that in these cases the habitual congestion of the head may lead to that chronic kind of inflammatory action in the lens, which eventually terminates in cataract.

From what has been said respecting the varieties of cataract, may, in some measure be explained, the different results which attend upon the operation for its cure; but before describing the various modes of operating, now in practice, it will be necessary to point out certain circumstances which absolutely forbid the operation, or render its success more or less doubtful. The prognosis is always unfavorable,

1. When the cataract has been preceded by severe and distressing pain of the head. This may even have abated for a considerable time, but it is always sure to recur immediately after the operation.

2. Where the patient has been long subject to rheumatic pains

* Vid. Nosographie Chirurgicale, Vol. II. p. 92.

of the joints, &c. If the operation be decided upon in these cases, we should endeavour to divert the excitement from the eye, by employing sinapisms to the calves of the legs; but, in no instance, attempt to operate in the spring or autumn, as the changes of temperature at these seasons, always increase the disposition to this complaint.

3. Where there is any tendency to erysipelas of the face.

4. Where the patient has suffered in his youth from frequent convulsions, or attacks of epilepsy. Professor Beer says, he has known such convulsions excited by the operation for cataract, after they had ceased for thirty years and longer. This accident has occurred to him twice in his practice; but fortunately, in both cases, after couching, so that no great harm resulted.

5. Where any particular diathesis is present, at the same time, in the system.

6. Where the patient has suffered under frequent and violent inflammation of the eye.

7. Where the patient can no longer distinguish the different grades of light and shade.

8. In all hypochondriac or hysterical patients. Professor Barth operated in a case of this kind, on both eyes, with the most complete success. A short time, however, after he had finished the operation, he was sent for in great haste, to visit his patient, and he was not a little surprised, on entering the chamber, to find him out of bed, the window-shutters thrown open, the bandages off his eyes, standing before the looking-glass. On examining the eyes, it was found, that a great part of the vitreous humour had escaped, and violent inflammation succeeded, and the operation was, of course, rendered futile. In another patient, operated upon by Professor Beer, the bandages were drawn off, half an hour only after he left the room; although he had given the strictest injunctions to the person to keep himself quietly in bed. For these reasons, couching is always the safest practice in these cases.

9. Where the cataract is complicated with some other local affection, as amaurosis, hydrophthalmia, glaucoma, synchisis,* and

* The operation is not even practicable, where there is synchisis or dissolution of the vitreous humour: for as soon as the first incision is made

the other diseases we have noticed, when speaking of the various complications of cataract.

(To be Continued.)

A Case, where Lumbrici Teres were discharged through the Abdominal Integuments, with some Observations. By P. Macaulay, M. D. of Baltimore.

While it is indispensable to a correct practice, that the laws which govern the animal economy in the healthy state, and the general changes which occur in disease, should be well understood; it is particularly incumbent on those engaged in the practice of medicine, to note with precision, those remarkable cases which not unfrequently present us with deviations from the ordinary results, which our pathological information has led us to anticipate.

The records of clinical medicine abound with facts interesting to our science, whether they be viewed as leading to a correct pathology in disease, or in explaining some of the most remarkable phenomena which take place in the animal system. How often do we observe a train of symptoms, follow morbid change of structure in one part of the system, which completely divert our attention to parts far removed from the seat of disease; and how much are we sometimes deceived in the termination of some surgical diseases.

In the infancy of medical science, while yet it struggled with superstition and ignorance, and was secluded in the temples of the priests, we learn that it was customary to expose the patient on the high way, that those who passed, observing a malady from which they had obtained relief, might make known to the afflicted

through the cornea, the whole of the fluid issues out, as there is no longer any membrane to contain it; and the eye is irretrievably lost. It is equally impossible to practise couching, or the breaking up of the lens, in such cases, as on the first puncture through the coats, the vitreous humour squirts out, as from a syringe. The coats collapse, and all attempts to reach the posterior chamber, (if any may be said still to exist) will prove of no avail.

the remedy. Few could have been assisted by means so precacarious, and thousands must have been the victims of vulgar prejudice and experiment: but such was the first dawning of medical science; and through it we see the first application of observation and experience, although entrusted to the community at large, to the relief of the maladies of mankind. In the same age, so dear in its recollections to posterity, he who first attentively observed the operation of physical agents on the animal structure, both in its healthy and morbid state; who noted facts in relation to diseases and their anomalies, and drew on the boundless kingdoms of nature for their relief, was deified by his countrymen, and had conferred on him, by the consent of after ages, the appellation of "Father of Medicine." Since that era, medicine has continued to advance as a science, and though obscured by the darkness which hung for so many centuries over Europe, the inquiries and observations of modern times have exalted it to the first rank among the useful professions. Coequal with civilization, its improvement has been much assisted, in every stage of its progress, by the important inductions which have been drawn from the *individual histories of disease*, which are recorded in its annals.

Considerations, such as these, have induced me to write the history of the following case, believing that its novelty will, at least, attract for it some attention. The subject was Mary Young, a patient of our city dispensary; fifty-six years of age, of a thin habit of body, and delicate constitution. I visited her on the evening of the 30th August, 1816. She complained of violent spasmodic pain in the abdomen; slight pain in the head, brought on from occasional efforts to vomit; the bowels had been constipated for the four preceding days, during which she had suffered, at intervals, from colicky pains; but the symptoms had not been so urgent, till within the last ten hours. The excitement of the system was unequal; the skin clammy and cool; much thirst; tongue, in the centre, covered with a dirty yellow fur. The pulse was feeble and slow. The patient stated that she had been the subject of frequent attacks of colic within the past six months. I proceeded, as is usual with me in all such cases, to examine the abdomen. In the right iliac region, midway, but a little above a

line, drawn from the superior anterior spinous process of the ilium to the os pubis, a hard, irregular tumor, from which red streaks of inflammation were diverging, presented itself. Pressure gave not more uneasiness than is commonly felt in ordinary cases of phlegmon. After the most attentive inspection of this tumor, considered in every relation to the prominent symptoms of the case, as described above, and having satisfied myself that no descent of the intestine had taken place in the usual outlet, I left the patient, under the full conviction that it was altogether adventitious. The result, however, will show differently.

Strong purgative enemata were ordered to be given at proper intervals, until the lower bowels should be fully relieved. Ten grains of calomel, with a portion of opium sufficient to relieve the spasmodic pain and calm the irritable state of the stomach, to be taken every three hours, until the alvine evacuation should be established: to the tumor, the common saturnine application. With these directions, I left the patient.

On the succeeding day, 31st, much relief had been obtained by the free evacuation of the lower bowels from the enemata; the calomel had acted but partially in evacuating the stomach and lesser intestines. The efforts to vomit had ceased, on the exhibition of the second or third dose of the calomel and opium; some pain and uneasiness was yet felt in the abdomen. The excitement was in a degree restored to the surface; the pulse more full; the tongue unaltered. The inflamed appearance of the tumor continued with a visible enlargement of its surface, in the direction of the fibres of the external oblique muscle. As it was evident, that the obstruction which existed in the intestinal canal was not totally removed, a repetition of the calomel without the opium was ordered, the purgative effect to be assisted by doses of castor oil, in such quantities as the patient's stomach would receive and bear, at proper intervals; injections of a solution of glauber's salts and molasses to be occasionally given. These means procured, by the evening, several small and slimy evacuations. The stomach, which had become irritable again, was calmed at bed-time by a moderate anodyne, and the saturnine applications to the inflamed tumor, which were complained of on account of coldness,

was changed, with much comfort to the patient, for warm fomentations of chamomile.

From the 1st to the 5th September, but little change in the symptoms could be observed. Daily, but partial fecal discharges were obtained by the ol. ricini, with the aid of injections. The calomel was omitted after the second day, in consequence of the ptyalism with which the patient was threatened. The tumor had continued to enlarge, and now occupied that portion of the abdomen covered by the right external oblique muscle; it very much resembled idiopathic inflammation of the muscle itself. No resolution being effected by the means hitherto employed, an epispastic, large enough to cover the inflamed and swollen part, was applied. The strictest regimen had been observed during the treatment. Fluids, such as water gruel, rice water, barley water, and occasionally, weak chicken broth, formed the only nutriment which the system had received. The age, situation in life, and habits of the patient, from the debility which had ensued, now called for a more invigorating diet. Yet, the local inflammation existed, and the obstruction in the bowels was not overcome; it was evident that the utmost caution must be observed, to prevent an increase of the inflammatory action in the parts concerned on the one hand, while, on the other, it was necessary to give support to the system; this was best effected by nutritive enemata frequently administered through the day.

6th September. The blister drew well; ordered it to be dressed with ung. sabinæ. The general treatment to be continued.

7th September. On removing the dressings from the tumor, I observed three sphacelated spots, one larger than the fourth of a dollar, about the centre of the belly of the muscle; two others, which were smaller, somewhat lower down. Gangrenous vesicles around these spots, indicated an increase of the disease. The carrot poultice was directed, with injections of decoct. cort. peruv. and wine and water freely through the day as a drink.

On the 8th, the gangrene had extended itself around the spots observed yesterday; several other spots, in the first, or gangrenous stage, are evident to-day. Powdered charcoal was added to the

poultice to correct the fœtor, and stimulating nutriment more freely given.

On the 9th, the largest spot had sloughed, and to my astonishment, brought to view the body of a lumbricus teres, inserted in the muscle. The worm seemed to have advanced obliquely upwards beneath the tendons; to have pierced the muscle about the centre, and again to have dipped, so that the part visible stretched like a bow, across a muscular space of two inches. Struck with so peculiar a case, I did not proceed to extract it, but ordering a continuance of the remedies, visited the patient on the succeeding day, the 10th, accompanied by my friend, the late Doctor Middleton. A further separation of mortified parts had taken place, and the worm was now to be seen at another point two inches lower down. From the upper hole, I extracted it with a forceps, and then endeavoured to ascertain, with a probe, where the peritoneum had been perforated. It passed downwards, from the lower sphacelated spot, two inches or more, when some obstruction offering, the examination in that direction was discontinued. Passed upwards, from the place where the body of the worm was first seen, the probe went within an inch of the lower false rib, and inwardly to the tendon of the internal oblique, where the head of the worm had lodged. It was alive, and measured eight inches, within a few lines.

The treatment of yesterday was continued.

On the 11th, the body of another worm was discovered by a slough from the integuments covering the tendon of the muscle; it was extracted, and measured six and a quarter inches. From the sloughed part, this worm had made its way up about two inches. On pulling out the lower portion, I observed that it was tinged with bile. A probe now passed easily downwards, and entered the intestine about one inch and a quarter above Pouparts ligament, at the point where the inflammation and swelling was first observed. Adhesions, from inflammation had, no doubt, taken place, between the intestine and peritoneum, as the canal thus formed was impervious on the sides, down to the cavity of the gut.

Other mortified spots had suppurated, but no further appearance

of worms could be discovered. Some uneasiness being felt in the bowels, an ounce of ol. ricini was directed to be taken.

12th September. I was much gratified by the effects of the ol. ricini. It produced several free evacuations, with which were brought seven large lumbrici, alive and dead. The patient now said that she felt, for the first time, that her bowels were relieved. Previous to the operation of the medicine, a worm emerged through the canal probed yesterday, and was drawn out by one of the attendants. The most unpleasant consequences were now to be apprehended from the gangrenous disposition in the inflamed integuments of the abdomen. I directed an epispastic, somewhat larger than the inflamed surface, to be applied. Encouraged by the complete relief of the bowels, the bark was administered liberally, with wine and a more nutritive diet.

The mortified parts were kept moist with yeast, and the epispastics re-applied daily, from the 12th to the 15th. This treatment was attended with the happiest effects. The gangrene was speedily arrested; the dead parts thrown off, and healthy granulations sprung up in every direction, to supply the waste which had been sustained.

Blisters, as a remedy for gangrene, are eminently entitled to the attention of the profession. I had first the pleasure of learning this remedy from the lectures of our justly distinguished countryman, Dr. Physick. To him are we indebted for the first introduction of this best of all local applications; and I here bear testimony of its success in the practice of the army, during the late war, and more recently in my professional avocations in this city. In every instance, where the death of the part has not been brought on by a diminution of the power of life at its seat, or when not attended with some specific diseased action, the result of the application has been uniform—to arrest the gangrene.

This patient continued to do well; constipation of the bowels was carefully avoided by the occasional exhibition of ol. ricini: no further discharge of worms took place. The tonic treatment was observed, and she recovered rapidly. I met her in the street about four months since, and was informed that her health had been uninterrupted since the occurrences here related.

The presence of lumbrici in the intestinal canal, are known not to be always productive of injury; but it is equally true, that they often give rise to some of the most formidable diseases. Fever, mania, epilepsy, enteritis, marasmus, gastrodynia, amaurosis, with all their dreadful symptoms, are sometimes caused by their presence. They have been known to invade the liver in man and some of the inferior animals. I have seen, in the collection of morbid anatomy of Professor Gibson, a human liver, completely occupied by them. Passing through the ductus communis, they formed a nest where the great vessels enter the gland, from thence were seen to pass in every direction, in vast numbers, through it. In a viscus of so much importance in the animal economy, it is somewhat astonishing that life should have been supported long enough to produce so great a number of animals which preyed on this gland, unless its substance favours, very rapidly, their increase.

Hildanus observes,* that round worms are sometimes found knotted, and in a large ball, safely lodged in the ileum, whose coats they often corrode so as to penetrate into the cavity of the abdomen. It may be doubted whether they ever escape, except from ulceration caused by disease of the coats of the bowels; this, no doubt, does occur; yet, I am inclined to the opinion, that under certain circumstances, they do perforate the intestines by a process somewhat resembling suction, performed by fixing their triangular mouths on the inner coat. The following case, met with in one of the inferior animals, seems to establish the fact. Some years ago, on dissecting a dog, whose death had been occasioned by the accumulation of round worms in the intestines, I found that they had pierced the bowels in many directions. Some were yet struggling half through, while others had just buried their heads in the coats of the intestines. It would have been very reasonable to have concluded that this occurrence had taken place post mortem, had not the effects of their escape before death been too evident. It was seen in inflammation of the peritoneum, and a collection of pus between the peritoneum and bowels, in which were several worms. My friend, Dr. Frick, has kindly furnished me with notes of a case which he attended the examination of, at

* Vide Hild. in cap. 1. Obs. 57.

the Hotel Dieu, in Paris. The patient had died of peritonitis. On opening the abdomen, an immense quantity of matter was found deposited between the peritoneum and intestines, and on a careful inspection, several lumbrici were discovered in this fluid. In one of the convolutions of the intestines, was a small hole through which they had escaped. M. Dupuytren was present, with his class; he stated, that there could be no doubt these worms were the cause of the peritonitis, and of the final death of the patient; in proof of which, he adduced several instances of the same fact; where lumbrici had made their way through the bowels, into the abdominal cavity, giving issue to the gazes, mucus, and often the feculent matter which they contained. In other cases, he had found these worms between the coats of the stomach, or intestines, producing tumors in these parts, which when cut into, not unfrequently exhibited whole nests of these vermin. These cases were very aptly compared by M. Dupuytren to that disease in the oak, produced by the cynips, which gives us the nutgall.

About three winters ago, in dissecting a subject of twelve years of age, in this city, the remarkable cause which occasioned his death exhibited itself. Intus-susceptio of the ileum had taken place for about two inches; around that part of the gut which had descended, several worms had closely wound themselves; the consequences were, obstruction, inflammation, and gangrene. Fortunately for our art, such instances as these are rare; but they sufficiently shew how difficult it is to ascertain, in many cases, the causes and seats of disease. I must leave the practical inferences which may be drawn from these observations, for those whose notice they may be so fortunate as to attract.

On Cholera Infantum, or Summer Complaint of Children. By C. D. Meigs, M. D. of Philadelphia.

Cholera Infantum ordinarily makes it appearance, in Philadelphia, earlier or later in June, as the weather is hotter or cooler

in that month, and continues, until October, to demand a considerable share of the attention of physicians who are much engaged in prescribing for the lower class of inhabitants, in whose confined, ill-ventilated houses, the children are exposed to the destructive effects of improper diet and clothing. It is, however, by no means confined to this class of persons; for so general and pervading is its influence, that those children who enjoy the advantages of the most prudent and anxious regulation, can only be said to be less exposed.

It mostly seizes children between their 5th and 20th month; hence the *second summer*, a very common expression among us, is considered by mothers, as a most critical period, simply in view of this disease. So much is this the case, that *the second summer* has become a familiar expression, for a period of great hazard and alarm; it is the most pressing period of dentition, which, as well as hot air, confined houses, bad diet and clothing, ought certainly to be ranked as one of its most active causes.

I am well aware that many most respectable physicians, depending on the ability of nature to effect her legitimate purposes, reject this idea altogether; but it is not a proof of the inefficacy of her general plans, that the natural operation of developing the tooth, should often prove a cause of disease and death, any more than that the natural and necessary act of parturition should cause the death of many women and their offspring; so that the idea of its being a necessary act, or one connected with the health and comfort of the individual, does not carry with it the conviction, that it must be a safe one. But in the case under consideration, the facts, that those children who get their teeth regularly, and in proper season, are more rarely affected; that of those who perish, a large proportion suffer during the period of dentition, and that few are attacked with cholera after that period is over, ought to be decisive.

In our mortality bill, for 1818, I find the record of one hundred and seventy-three cases of cholera infantum; of these, 105 died under one year; between one and two years, 68. The whole number of deaths in children under 2 years of age, in all diseases and accidents, with the exception of *still born*, was 668. This

statement leaves a proportion of one in four nearly, of deaths from cholera infantum.

In 1819, we lost 1185 children under 2 years of age. If we leave out 145 still born, we have 1040 for diseases and accidents; 225 children, under 2 years, died of cholera, so that in that year, not quite one in four died of the disease. I presume, that if all the cases which are truly dependent on, or connected intimately with our disease, were reported, we should have a mortality from it alone, of nearly one for every three cases. This is enough to establish, at least, the high importance of the affection; and, in view of the above circumstances, the strongest interest must be excited to investigate its history and treatment.

To those who object to dentition as a cause of summer complaint, on the ground that it is only met with during the summer months, while teething goes on through all seasons indifferently, I beg leave to answer, that the cutting of the teeth, in all seasons, is commonly attended by symptoms which characterise, in some degree, our disease: I mean the obvious marks of an imperfect and vitiated digestion, such as looseness of the bowels, stools of unnatural colour and smell, slight fevers, &c. from which the winter does not preserve all teething children. Does not the whole appearance of a teething child denote the considerable irritation under which it is labouring? See it restless, dissatisfied, fretful, with its gums swelled, broad, and evidently painful; thirsty, sleepless; with cutaneous eruptions; cough, and commonly more or less of fever.

I am well convinced that all these circumstances, taken in connection with the vitiated atmosphere of a populous city, in the hot season, are sufficient to lay the foundations of a disease which exerts such a fatal sway among us.

It is a general principle in pathology, that increased excitement can not be long continued in the human body, without destroying that equilibrium of circulation which is absolutely necessary to the healthy condition of the frame. When the balance is lost, determinations are generally taken towards the larger trunks of vessels. I do not speak here of the aorta or vena cava. In children, these directions are principally given towards the brain or hepatic system. It is on this theory that I would build a history

of the cholera convulsion and hydrocephalus, which are so fatal here, for I believe that most of these diseases depend on loss of balance, or congestions of the blood-vessels in those parts which are the seat and throne of the peculiar affection.

The hepatic vessels, and their connections in young children, are more exposed to these accidents than in adults, from a variety of causes, such as the large proportion which they bear to the rest of the system, their sluggish circulation, their great complexity, the great irritability of the nervous system in that tender age, &c &c.

In the hot months, the tone of the skin becomes exhausted, which is a natural consequence of the incessant demands on its great and important function of perspiration. It acquires an increased sensibility to the morbific impressions of sudden changes of temperature; becomes dry and husky; no longer performs with regularity, its very important share of the elimination; and those refuse or excrementitious products, of our perpetually decomposing bodies, which naturally are excluded by the skin, are either retained, or obliged to seek their ejection at other emunctories. These facts cannot be looked on as connected with the exploded doctrines of concoctions, depurations, &c. but they are natural deductions from a fair view of the circumstances. The natural consequence of the condition of the skin, of which I have spoken, must not be overlooked. I mean, an abundantly increased secretion of fluids in the intestinal canal, in obedience to a general rule of the animal economy observed by Bichat, who always observed with precision. This general rule is, that those diseases which are characterized by an increased flow of secreted fluids, are remarkable for a diminution of the products of exhalation; a fact which every one has had occasion to confirm, as well as its converse, for the predominance of one set of functions implies a minus state of other sets. Thus, the skin ceases, in a great measure to act; the kidneys are not rendered more active; but on the contrary they are less so, and hence, the alimentary canal must become diseased by a load of crude matters, rapidly, and therefore imperfectly secreted.

In general, the cases of cholera which I have seen are not enti-

tled to that very distinctive appellation; for I have rarely seen any free discharge of bilious matter, either by vomiting or stool, though I would not have it understood, that I have never seen any bilious discharges in the disease. It would seem, that in violent cases the portal system is so charged with blood, that a complete apoplexy (if I may say so) exists in the liver, and that the organ is so oppressed by this condition, as to be unable to relieve itself by a free secretion: in the same manner as it happens to the brain, which loses its functions when in the same state; the kidneys also, do not secrete abundantly when inflamed; though in this last case, the analogy is not a perfect one. I believe that this apoplectic state of the liver is the cause of most of the sudden deaths in the complaint. I know we are all accustomed to hail the return of an abundant bilious secretion as the best evidence of a better state of things to our patient; even after the disease has existed three or four days, the liver is rarely able to afford much relief by performing its proper function, and many children for six weeks or two months, do not pass a stool, in the immense number of those which they do pass, in which more than a very slight trace of bile can be found. The practical inferences from these considerations, will be discussed when I speak of the treatment of cholera.

Symptoms.—As most of our patients in this disease are teething, we generally have from the mother an account of the symptoms common to that state: but we are often told, that a child in full and vigorous health is seized with vomiting, in which some slime is brought up, though mostly nothing is ejected at first, but the milk in a very acid state; bile, as I have said, does not often appear. Most commonly, however, a looseness of the bowels is the first evidence we have of an attack. The matters discharged are principally composed of an insufferably offensive light-coloured fluid, so thin as to run through the diapers like water, scarcely tinging them with a dingy yellow; this is accompanied, sometimes, with nothing but a little white mucus. In other cases, the mucus is mixed with a whitish pulpy substance, with here and there a few streaks or patches of green or yellow matter; but these latter bear no proportion, in point of quantity, to the other. Sometimes the stools are green at first, but a few days are sufficient to establish

the watery and mucous stools, and until the recovery of the patient, they maintain this character, rarely by an uniform yellow, or even green colour, indicating the presence of much bile. I have seen white stools (though not clayey) passed several days in succession.

Since the other products of intestinal secretion are generally white, we know from the prevalence of that colour, that the fæces have not been mingled with bile; the yellow, brown, black, or green colour of which, is easily recognised. The whole course of the disease shews a great deficiency of this important fluid.

The pulse is usually small and very frequent, though sometimes full; the tongue is white; but I have seen it, in an early stage, dry and polished: its appearance is often natural. In 15 or 20 days aphthæ often make their appearance.

The head and body are very hot; the arms and legs cold. The skin has a shrivelled look, particularly on the inside of the thighs, which are remarkably shrunk and wasted. This appearance of the upper and inner side of the thighs is very striking, and is an invariable attendant of a severe case. It depends on the very rapid absorption of fat from that part, which leaves the skin so relaxed as to hang round the inside of the limb very loosely. I think the restoration of an even regular surface here, is one of the best indications of steady convalescence.

The patient has a fretful discontented moan; he is very rarely blessed with a night of good sleep, and the eye-lid is found half closed, shewing the filmy dull-looking cornea beneath; debility and emaciation come on with very rapid strides, so that 24 hours will sometimes produce the most alarming languor and exhaustion. The vomiting I have not found an urgent symptom, that is, it is not urgent as in the cholera of adults; the patient, after the first day, seldom puking, except he eats or drinks too much; and then it is done without much retching or straining.

The stools, in many cases, have amounted to 15, and even 30 in 24 hours; but become less frequent as the disease assumes a chronic character. They are often preceded by great pain from griping and tenesmus. Many children suffer from the most intense thirst, so that they seize the cup with a vehemence which is truly distressing. The temper is highly irritable; sometimes the child

tears itself and attempts to bite its nurse in these paroxysms, which approach to a maniacal state. I believe that a large proportion of cases terminate in hydrocephalus; and these paroxysms of passion and distress indicate its approach. In the 2d vol. of Med. Chir. Trans. is a case by Cooke, in which he says, this is the fourth case of hydroc. which I have examined in twelve months, all of which were accompanied with affections of the liver. This observation confirms my opinions concerning the pathology of the disease in question, which hardly needs any confirmation, for we have all observed, in affections of the liver, the most convincing analogies. In icterus, who ever saw a patient with a moist skin, till the disease began to give way. In scirrhous affections of the same organ, the skin is dry; so also in acute inflammations we find the same relation existing between the functions of the liver and skin.

The dry state of the skin, which is so troublesome a symptom, is entitled to a large share of our attention, in as much as its removal must have great weight in the restoration of the functions of the liver and alimentary canal.

From what I have said, it will be understood that I wish to maintain that cholera infantum depends principally on a loss of the healthy function of the liver, and that most of the collateral symptoms may be traced to the pervading influence exercised by that, over the other important organs of the body; that it consists, not in an organic change in the structure of the organ, but probably in a congested state of the whole system of the vena portæ.

But my limits will not permit me to dilate on the whole pathology of this interesting disease; enough has been said to invite the attention of physicians to it, and it is to be desired, that some competent practitioner may be induced to give us an ample account of it.

I will now pass to the consideration of those methods which are to be employed in its treatment.

Our objects are, in the first place, to arrest the vomiting and looseness.

2d. To procure a more fresh distribution of the circulating fluids.

3d. To restore the tone of the general system.

Many slight cases connected with a tedious dentition, are easily removed by gentle evacuant medicines, and by cutting down to the advancing tooth. Many children may be saved by an early adoption of this method, from a severe illness. The liberation of the tooth destroys one source of severe irritation, and the bowels being at the same time unloaded, leave the system disposed to renew the vigorous actions of health.

Castor oil, with a few drops of laudanum, does not lose its cathartic power, and suits the peculiar condition of the stomach, where little or no vomiting has come on; it relieves pain, and procures feculent stools; hence, I have found it a very useful medicine in the slighter forms of summer complaint.

The antacid quality of magnesia indicates its employment, and when given alone, or with a small quantity of rhubarb, it is useful and proper. Rhubarb, too, is a very fashionable medicine in this disease, and, doubtless, is of very great consequence in the treatment of it. I beg leave to suggest the use of infusions in cold water, because I believe they partake more of the character of a tonic than the infusion in boiling water.

I have never made use of any neutral salts in such cases, because I was fearful that they might increase the watery secretions, which are already too abundant, and those medicines I imagine to be most appropriate, which, by acting more slowly and uniformly on the whole tract of intestines, produce the completest evacuation of their contents.

I am not inculcating the doctrine which is so universally applied at this day, I mean the doctrine of purgatives. I apprehend they will do mischief if carried to any considerable extent; yet, that it is absolutely necessary to keep the bowels free, there can be no doubt. Our chief dependence should be placed on those means which are calculated to restore the lost or vitiated secretion of the liver. I say lost, for any influence we can exert to ward its increase, almost universally answers a good purpose. A copious bilious stool hardly ever takes place that we do not find the patient better. His spirits are improved, and his skin loses

its harsh and husky appearance, regaining some moisture, and acquiring a more uniform temperature, and livelier hue.

Mr. Abernethy deserves great praise for his treatise on the constitutional origin and treatment of local diseases, in which his views of the alterative action of medicines are invaluable. I believe that it is on his observations, and on Dr. Ayre's of Hull, that is grounded the practice so general among us, of giving very minute doses of calomel.

Calomel is given here in doses of one-twelfth part of a grain every hour, two, three, or four hours, as circumstances may require, with the happiest effects. It is generally combined with about the same proportion of opium and ipecacuanha. This combination, carefully administered, almost always brings down a secretion of yellow bile. It procures sleep, softens the skin, and gives the most flattering manifestations of an improving condition for the patient; though, it is not used with an uniformly successful result, for while the original causes of the disorder remain, he relapses into the same condition of langour.

Calomel has unquestionably a *specific* relation with the liver, by exciting in it a grade of action compatible with its mode of vitality. When properly administered, it is followed by an increase of biliary secretion, and this secretion will generally be of a better quality, and as the bile is the natural stimulus of the intestines, it improves the digestion, while its flow relieves the congested vessels. Hence the improvement consequent on bilious stools in our disease, as in the stomach, by a diminution of irritability; in the bowels, by a more regular contraction; and in the skin, by a more vigorous determination to, and circulation through it.

In using the calomel, I would not depend on it alone. If we can produce its alterative effect, it is enough; we can always procure the regular evacuation of the bowels by oil, magnesia, rhubarb, &c.

Let it be given in the small doses above mentioned, they are more efficacious than large ones, and do not place the glands of the mouth in so much danger.

Injections are proper to relieve the painful efforts of the rec-

tum: they may be well used five or six times a day, if composed of thin mucilaginous materials; at night an addition of laudanum is useful.

Laudanum is better given in this manner, than when taken in the stomach; it does not weaken that viscus by its action, thus prolonging the disease, which may be concealed by it, but not cured; doubtless, it will be often necessary to administer it by the mouth, but I only mean to deprecate an attempt to cure the cholera, or to depend in a great measure for its cure on laudanum.

I have never used emetics, and can therefore say nothing of their merit in our disease.

Blisters to the epigastrium are more effectual, in cases where the vomiting is obstinate, than spice plasters or other weak rubefacients.

Fomentations of the belly with infusions of bitter herbs are very useful.

Preparations of chalk occur to every one as of some value in the case. Where a disposition to coma was manifested by the unclosed eye, the stupor and heaviness, the application of a blistering plaster to the epigastrium has in two instances been attended with excellent effects.

By these and such other means as are indicated, we can generally fulfil our first purpose; which is to arrest the vomiting and looseness. The second is to procure a more equable distribution of the circulating mass.

The cold bath seems never indicated in this disease. The warm bath, on the contrary, is certainly a remedy of great promise. The increased action of the extreme vessels excited by its use, is calculated to take off the venous plethora which has been described, and the frictions which should succeed its employment are calculated for forwarding the same end.

In the chronic stage of the disease, I believe that the child should never be washed in cold water, but brandy or rum alone should be used to cleanse it. I have seen the most pleasant effects from the use of these things.

While on the subject, I will notice the infusion of the white oak bark, which I have seen used as a plunging tepid bath, once

or twice a day, with a rapid amendment in one instance, and a perceptible improvement in another.

The clothing is an important part of the treatment. A shirt of soft fine flannel is an indispensable article, and the physician should examine it, with a view to have it well fitted to his patient; for, without this precaution, many of the flannels are made of an unnecessary size and weight, which only add to the distress and uncomfortable feeling of the patient. The over-dress may be made of any light stuff, and should always have sleeves down to the wrists. The neck should not be left exposed. The legs should be covered with light stockings, reaching up to the knees. A scrupulous regard is to be paid to neatness and cleanliness in the arrangement of the clothes.

Having done all that can be effected in this manner, recourse must be had to such remedies as are calculated to restrain the secretions of the intestinal canal. A great variety of articles are at hand for the promotion of this object. The dewberry root, logwood, catechu, geranium maculatum, cherry bark, heuchera americana, kino, creta, &c. are among the principal of such remedies. The geranium maculatum, boiled in milk, is used by some practitioners, and I have been informed with much success. The late Dr. Barton mentions this remedy as having been introduced by the late Dr. Hand. Notwithstanding all that can be done, many cases run on to a chronic form, and yield only when the season of frost and cold arrives. The diet should be particularly attended to, especially when the disease assumes a chronic form. Preparations of arrow-root, sago, tapioca, &c. with a proper admixture of new milk, form the most suitable diet for children affected with this complaint. G. arabic, with lime-water and milk, has been prescribed with much advantage, when the vomiting has been troublesome. Coffee, strong soups, and meat of every kind, are inadmissible. Very ripe fruits, if given at all, are not to be trusted but in very small quantities.

Exercise in a carriage has an excellent effect, in restoring tone and vigour to the system, and should always be ordered for those whose situations admit of it.

After all, I am well satisfied that no plan of treatment can stand

in competition with that of sending children to the country. No child that is sick, or threatened with sickness, should be allowed to breathe the unwholesome air of the city. Few of them perish when removed early into the country, and most of them are rapidly restored to health. So well was the importance of this step impressed on the philanthropic mind of Dr. Wistar, that he exerted himself successfully for the establishment of an hospital for sick children, at Bush Hill.

New and superior mode of making Mercurial Ointment.

In "Thompson's Annals of Philosophy," &c. &c. for October and November, 1819, is a paper on the oxides and salts of mercury, by Mr. Donovan, of great practical utility to the chemist, physician, apothecary, and their patients. He first treats of the action of the pure alkalis and alkaline earths on the salts of mercury; 2d, on the analysis of the protoxide and peroxide of mercury; 3d, on certain properties of the mercurial oxides. In the second part he treats, 1st, of the combination of the nitric acid with the oxides of mercury; 2d, 3d, and 4th, of the combination of the sulphuric, muriatic, and acetic acids with the oxides of mercury. The results of his experiments are given in the conclusion, and are to this effect; "that there are no grounds for even supposing the existence of any other oxides than the black and red, or, what is the same thing, the yellow. He shows that in all the salts, the metal unites to the acid, so as to produce a neutral and a subsalt with each oxide, the catalogue being completed by the subsulphate, submuriate, subacetate, and suboxymuriate.

New Mercurial Ointment.—In an attempt to obtain black oxide of mercury, he found that 60 gr. of metal, after 40 hours trituration, afforded but 6 grs. of oxide. This fact, considered with the short time that is bestowed on the extinction of the mercury in the mercurial mills, excited his curiosity to examine the state of the mercury in that mercurial ointment which is used in the cure

of the venereal disease, and of which the nature has been so much disputed. He found that by keeping 4 oz. Troy of mercurial ointment, six months old, at a temperature of 212°, it separated into two strata. When cold, the upper stratum was separated; it was of a light grey colour. The under stratum was exposed to 212°, on blotting paper, which soaked the remaining lard. The very heavy residue was triturated with a little magnesia, and it almost instantly afforded a quantity of mercury, weighing 495 gr.: mere trituration with magnesia afforded 225 gr additional. The earthy mass was put through a variety of processes, which gave a quantity of globules; they may be rated at 60 gr., although he could not collect them all: a little oxide also appeared. Thus out of 960 gr. of mercury, which the 4 oz. of ointment originally contained, there were recovered 770, which leaves 190 gr., the quantity of mercury apparently oxidized, or 47½ to each ounce. But the quantity is really less. The grey ointment, which constituted the upper stratum, appearing to contain oxide of mercury in a state of chemical combination, and believing that the lower stratum, being metallic mercury, could have no effect on the animal economy, when introduced into the circulation (stomach), he conceived that the medicinal effects of mercurial ointments must depend on the small quantity of oxide with which the fat is chemically combined. He therefore determined to try if this grey ointment would affect the human body in the same manner as the common mercurial ointment. He provided three females, whose situation required the use of mercury; each rubbed in a drachm every night. One was affected by the third rubbing, and the fourth rubbing put her under ptyalism. Another of the females, after rubbing three times, was so salivated that she spit two quarts in the twenty-four hours; and this in a less degree was kept up for ten days. The third was not affected until she rubbed six times, and then not considerably. They were all at length recovered.

Finding the ointment so very active, he conceived that, by forming a chemical combination between fat and oxide of mercury, in a very small quantity, the same result might be obtained. He therefore kept lard and black oxide of mercury at the tem-

perature of about 350° for two hours, continually stirring them. At the end of the process it appeared that every ounce of lard had dissolved and united with 21 gr. of oxide.

He tried the effects of this ointment on several persons, and found it to be as effective as the common mercurial, which contains almost twelve times more mercury. One drachm could be rubbed in completely, in from 6 to 10 or 15 minutes, while an equal quantity of common ointment will require 30 or 40 minutes. This to debilitated persons is a great advantage, the usual mode of friction being to such persons a most laborious undertaking. In but one or two cases was an eruption produced on the part rubbed, and this was inconsiderable; but, in the common mode, the pustules are often so painful and numerous as to preclude the further continuance of friction. The use of the new ointment is extremely cleanly, the parts being left scarcely discoloured, and this, where privacy is an object, is of great importance. It is also much cheaper than the old ointment.

For the preparation of this ointment it is essential that the lard be entirely free from salt: if not, calomel will be formed. The oxide may be prepared by decomposing calomel with a solution of pure potash, or, which is cheaper, by pouring a solution of nitrate of mercury into caustic alkaline solution.

Although the fat will scarcely dissolve more than three grains of oxide to each drachm, the quantity of oxide may be more, if necessary. The ointment that he employed was merely saturated. The oxide should be first triturated with a little lard, in the cold, to make the penetration complete. The degree of heat at which the combination is attempted is material. At 212° the oxide and lard will not combine; at 600° the oxide will be decomposed, and mercury volatilized; at 500° and 400° the oxide is partially decomposed, some red oxide being formed, and mercury reduced. The best heat is between 300° and 320°: it should be maintained at least an hour, and the ointment should be stirred till cold. This ointment has been used by many of the most eminent surgeons in Dublin, both in private and hospital practice. In the Lock Hospital, expressly devoted to syphilis, it has been extensively used, and is still under use. Mr. A. Johnston, in a letter

to Mr. Donovan, says that in eight patients a drachm of it was rubbed in perfectly dry in fifteen minutes; and it salivated in a shorter time than an equal quantity of common blue ointment.

Mr. C. Roney, and Mr. J. Shield, of the king's military infirmary, confirm his statement, and that of Mr. Donovan, as to the superiority of the new ointment, in the points mentioned.

I consider this discovery, with that of dissolving magnesia in aërated water, as two of the most useful modern additions to our previous stock of pharmaceutic knowledge; and the world are certainly greatly indebted to Mr. Donovan for his labours. I hope that the apothecaries of the United States will avail themselves of it, and that physicians generally will prescribe the new mercurial ointment, in preference to that made agreeably to the old formula, the inconveniences of which, we all have long complained.

J. M.

Case of Sudden Death in the latter part of the ninth month of Pregnancy. By J. Revere, M. D.

Thursday, July 28th, 1820. I was invited by Dr. Chapman, to be present at the examination of the body of a black woman, who died under the following circumstances. She was the mother of eight children, and was in the last month of her pregnancy with the ninth. She had enjoyed her usual share of health, until the preceding Tuesday, when she was seized with vomiting, and obliged to give up her work, which was that of a washerwoman. From the account of her mother, her indisposition seemed trifling, and she was much better the following morning. In the evening of this day (Wednesday) she complained of colicky pains, which induced her friends to send for a physician, who directed a dose of salts and sennæ, by which she was relieved. On Thursday morning she seemed quite well, but in the course of the day she experienced difficulty in passing her urine, which was easily relieved by some simple domestic remedy. In the afternoon, the

attendants supposed she was in labour, and sent for the midwife, who did not see her, but sent two pills, one of which was to be given immediately. Soon after taking the pill, she fell asleep, but awoke suddenly, and requested assistance. As soon as she stood up, there was discharged on the floor, in the opinion of the attendants, about a quart of dark, fluid blood. She was replaced in bed, and a physician sent for; but before Dr. Chapman arrived, the woman was dead.

We examined the body the same evening. The woman was short and corpulent; the abdomen very prominent. On examination, the os tincæ was found dilated about the size of a 12 cent piece, and the membranes entire, with a very slight oozing of blood from the os externum; the head presenting. On dividing the parietes of the abdomen, there was found a considerable serous effusion. The uterus was natural in its appearance about the fundus; but, where the cervix was in contact with the brim of the pelvis, there were patches larger than the hand, of a bright mahogany colour, between arterial and venous blood, which had the appearance of ecchymosis. The uterus, with the fœtus and membranes entire, were removed, and the remainder of the abdominal viscera were examined, which were healthy in their appearance. The unseasonableness of the hour, rendered a further examination impracticable. The uterus and its contents were taken away for a more careful and deliberate examination. As it was thought desirable to make a preparation of the parts, they were immersed in alcohol for three days, preparatory to a further examination. On making a crucial incision on the anterior part of the uterus, a large mass of coagulated blood was found accumulated at the fundus. The quantity, on removal, was thought to exceed four pounds. The placenta was found detached, except in a small spot.

The case seems worthy of being recorded, as it is certainly one of rare occurrence, and shews, in a striking manner, the importance of examination, post mortem. So completely was the blood retained at the fundus, that, even after the uterus was detached and agitated, and moved in various directions, not the least blood was observed to issue from the os tincæ; and the real cause of death was not suspected, until the uterus was laid open.

[We invite the candid attention of all our readers, and of the profession at large, to the following additional remarks of Professor Mott on his late operation. Our journal has been the vehicle of many severe strictures upon the conduct of that gentleman; it is but just that it should be the medium of his defence. It has always appeared to us strange, that Dr. Mott should so far disregard the attacks upon his character as a surgeon, as to remain perfectly silent. Our surprise is increased by the communication which he has handed us. If the state of the diseased parts is such as he describes it to be, there can be no doubt that the subclavian artery was really *aneurismal;* and by a simple description, properly attested, he might have completely satisfied the doubts of the profession, and rescued his character from reproach. But although Dr. Mott was not perfectly satisfactory in his former account, as it respects the precise nature of the case, we conceive that whatever he adduces, in further elucidation of this point, should be very candidly received and examined.]—Editors.

—

Further remarks on the case of ligature of the Arteria Innominata.
By Valentine Mott, M. D.

In my first communication of this case in the New York Hospital Register, for 1818, it is stated, page 50, that "*the subclavian artery internally and externally to the disease was pervious.*" To this it may now be added, that where the artery opens into the ulcer left from the wound of the operation, it appears not only pervious, but of the natural size, and the coats free from any diseased appearance. Externally, towards the axilla, the artery is somewhat enlarged in diameter, but exhibits no appearance of disorganization of its coats, either externally or internally. About an inch from the ulcer, or just as the artery has passed between the scaleni muscles, there is an irregularly shaped elliptical opening upon its upper side, large enough to receive the extremity of the fore finger. The edges of this opening are jagged and uneven, and the surface of the artery internally

is of a brownish yellow colour, to the extent of half an inch on the inside of the opening, and more than an inch towards the axilla. The internal coat of the artery has a rugous or puckered appearance, separated a little from the muscular coat, very friable, and evidently in a degenerated state. This opening of the artery communicates directly with the anterior extremity of the sac, which contains coagula; and upon removing these the surface of the sac is seen puckered, or thrown into a great number of little folds, giving it at first sight the appearance of containing a number of holes. This account is taken from the morbid parts before me; and the preparation has been seen and examined by Dr. Post, Dr. Hosack, Dr. Stevens, Dr. Watts, and others, who have authorized me to state, that they are satisfied as to the nature of the case.

New York, 9th mo. 1st, 1820.

Case of extraordinary maleformation of the Genital Organs. By Henry W. Ducachet, M. D. of New York.

In November, 1814, a man, aged about thirty years, was committed to the Bridewell of this city. He immediately revealed to the medical attendants of the house, a singular maleformation of his genital organs. His penis was imperforate, destitute of a prepuce, not more than an inch and a half in length, and had an excavation upon the upper surface of the glans. A little above the insertion of the penis was a tumour, about the size of a goose's egg, of a granulated appearance, and of a bright red colour. This tumour dilated and contracted irregularly. It was largest in the morning, and smallest at night. At the lower part of this tumour there appeared two foramina, of a size sufficient to admit a crow-quill, and from which the urine continually dribbled. He had a large inguinal hernia on each side. To defend the parts from pressure, he wore over them a tin plate, to which was appended a contrivance of his own, to receive the water as it dropped. His testicles were perfect, and remarkably large;

and he confessed that he was frequently tormented with an inclination for venery. It is probable that the umbilical cord entered at this tumour, as there are no traces of a navel. In consequence of his confinement, he was not able to keep himself perfectly clean; so that the neighbouring parts were considerably excoriated by the acridity of the discharge.

Dr. Hosack, of New York, has in his possession a plaister model of a lusus of precisely the same kind, which he took from a man who exhibited himself to the students of the university of Edinburgh, during his attendance at that school in 1792. Dr. Duncan has related several cases of the same nature; but the one just detailed is, perhaps, the only one that has ever occurred in this country.

REFERENCES TO THE PLATE.

A the penis. B the excavation on the glans. C the tumour. D D the mouths of the ureters, from which the urine continually dribbles.

REVIEWS.

ANALYTICAL DEPARTMENT.

Authoribus suus constat honos . . . . .
Nosque non judicis set indicis personam sustinemus.
BACON.

Medico-Chirurgical Transactions, published by the Medico-Chirurgical Society of London. Vol. X.—London, 1819.

We are not aware that there is any Medical Journal, which was ever conducted better, than the one whose title we have placed above. The society who publish it, is composed of the first physicians and surgeons of the united kingdom of Great Britain, and its contributors are not only the first men of Europe, but of the world. We propose, in the execution of our plan, to give analytical reviews of the more interesting periodical publications in medicine, and shall make it a point always to draw largely from the excellent work, the tenth volume of which we are now about to consider. As this volume has, however, been a considerable length of time in the hands of our countrymen, and as many of its more interesting papers have already been republished, we shall in this instance content ourselves with a very short summary of the information which it contains.

The volume commences with a paper by Dr. James Blundell on the Physiology of Generation. The experiments brought forward by the ingenious author are very interesting, and tend, in our opinion, to throw considerable light on the highly important functions which they are intended to illustrate. We conceive,

however, that the experiments adduced by any individual, in support of a particular theory, should be received *cum grano salis;* for the mind, however philosophical it may be, when it takes a certain bias, is too apt to overlook circumstances, which, if taken into consideration, would go in a great measure to overturn what before appeared demonstrated results. These observations are made, not with the view of throwing discredit on Dr. Blundell's experiments, but are merely employed for the purpose of encouraging a stern scepticism, which we believe is of the first importance for the prosperity and well-being of the inductive philosophy.

The experiments are performed with the view of proving, that the sexual excitement produces or brings into action, certain phenomena in the female, but that for perfect impregnation, it is necessary that there should be applied to the embryo productions of these phenomena, the male semen. The uterus in the rabbit* is double; the cavities of each are separate, and only communicate by their mouths opening into a single vagina. Experiments were executed by dividing one of the uteri, so as to destroy its cavity, and consequently to shut up the only passage by which the male semen can pass to the ovary of that side: when the wound was cured the male was introduced, and repeated copulation permitted. On examining the body of the animal some time afterwards, it was found that the uterus which remained undivided contained fœtuses, whilst the other was empty. But although no fœtuses were to be found in the divided uterus, still it was evident, that certain effects had been produced by the sexual connexion. The *corpora lutea* were as distinct on this side, as on the other, and the superior fundus of the uterus was enlarged, and had its vessels increased, as if preparatory for the reception and nourishment of an ovum. The second experiment had in view the prevention of the passage of the male semen to either of the ovaries. This was accomplished by cutting across the vagina, just where it is connected with the uterus. The animals when cured were introduced to the male, but although the sexual ex-

* Rabbits were employed in all the experiments.

citement appeared to be very lively, and copulation was frequently permitted, no fœtuses were produced. *Corpora lutea* were however formed in the ovaries, and in those instances where the male had been allowed frequent intercourse with the female, the uteri were considerably enlarged in substance, and contained in their cavities a quantity of a turbid fluid, which formed albuminous concretions at a temperature below the boiling heat.

Dr. Blundell's conclusion is, " that it seems probable, judging from the appearance related, that generation may be carried forward to a certain extent, although the access of the semen to the rudiments is intercepted. Under these circumstances the young animal cannot be formed, it is true; but *corpora lutea* may be generated; the wombs may be developed; and the rudiments, if we may judge from the facts already stated, may even be transferred to the uterine cavity, by the play of the fallopian tubes."

The second paper in the volume is by Mr. James Wardrop. It contains some observations on a mode of performing operations on irritable patients. Every surgeon who has had much practice in his profession, must have often felt it inconvenient, if not impossible, to execute proposed operations on certain patients from their great irritability. Such a case occurring to Mr. Wardrop, he was induced to bring the patient into a state of deliquium by opening a vein. Whilst in the state of fainting he extirpated the tumor, which he had been before foiled in removing.

The formation of hydatids in the frontal sinuses of sheep, is by no means an uncommon occurrence; in the human subject it is however one which is very unusual. Mr. Keate relates a very interesting case of a young lady in whom hydatids formed in these cavities. Miss Arnold, the subject of the case, had discovered, six years before consulting Mr. Keate, a small tumor about the size of a hazel-nut in the situation of the sinuses. It was from its commencement attended with a throbbing pain in the position of its base, but as this gave her little uneasiness, and its growth was hardly perceptible for the first three years, no attention was paid to it. For the last three years it increased rapidly in size, and she felt intense headachs, with occasional vertigo, dimness of sight, nausea, and tinnitus aurium. When the ope-

ration was performed, the 3d of April 1816, the tumor had acquired the size of a small orange. The integuments being removed, the bony covering of the tumor was exposed. After sawing around one-third of its circumference at its base, a portion of the bone being broken away by the *elevator*, a thin transparent membrane was discovered closely lining the bony case. When opened, a thin colourless fluid escaped, the cyst at the same time collapsing into the cavity. The finger was introduced, and the surface of the cyst was discovered to be irregular, and the internal table of the bone depressed below its natural level. Very injudiciously, in our opinion, the cyst was neither removed nor was the projecting osseus parietes of the tumor taken away. It was hoped that by the application of the *kali purum*, exfoliation would be produced and a cure accomplished. But, as we should have expected, this did not happen. The *embryo* hydatids which formed the irregularities on the inner surface of the cyst enlarged; after the wound healed a new tumor formed, it burst, and discharged hydatids. After repeated attempts at producing an exfoliation by the application of all the caustics in the materia medica, and after the young lady had borne the most excruciating tortures, for more than eighteen months, a second operation required to be performed, by which the whole of the osseous projection was removed. In the base of the tumor the cysts of six hydatids were found, they were opened, and destroyed by the application of the caustic. Four months after the second operation she left London cured, and has since continued to enjoy excellent health.

The idea of transfusing blood from one animal to another, with the views of giving strength and support to the weak and feeble, is not new. Our readers will find some interesting speculations on this subject in Miss Seward's letters. They were taken from conversations with Dr. Darwin. Other ingenious physicians have advocated this method of recruiting the exhausted strength, and Dr. Blundell, whose case we are about to consider, favoured the profession with an interesting essay on this subject in the last number of the Medico-Chirurgical Transactions.

A poor fellow with a stomachic disease, the true nature of which, scirrhus, not being ascertained by his physicians, was ad-

mitted into Gay Hospital. Unable from the state of his pylorus, to take any nourishment, he was at the time Dr. Blundell saw him, almost *in articulo mortis* from inanition. The veins were shrunk, and the want of blood appeared so palpable, as not to admit of question.

The operation of injecting blood into his veins having been resolved on, the cephalic vessel was exposed by an incision a little above the elbow, and a small opening about a line in length being made into it with a lancet, a tube was introduced into the cardiac extremity of the wound. Through this tube there was injected by an ounce and a half syringe in the course of forty minutes, 14 oz. of blood, which had just flowed from the arm of a healthy subject. No immediate visible effect was produced either on the feelings or pulse of the patient by this injection. In the course of the evening, the patient however experienced a salutary change; his body became warmer, and his pulse which had acquired nearly double its former size, beat with great regularity, eighty-eight times in a minute, which was the number before the operation was performed. He stated that "*he was better*," "*much better*," "*less fainty*," and he articulated with much more force. All these favourable symptoms combined during that night, and part of next day. The following morning at 8 o'clock A. M., he thought himself stronger than he had been the preceeding evening, and felt a degree of appetite which he had not experienced for three weeks before. His limbs and trunk were remarkably warm, and he passed a stool without an injection, which was very unusual with him. On the evening of that day, he however began to droop, and expired the following evening, fifty-six hours after the injection, apparently exhausted from inanity.

We have not space to allow us to enter into a critical review of this case, or to speculate on the interesting subject with which it is connected. This, we intend, in an original paper, to bring before our readers hereafter.

We had an opportunity several years ago, of seeing some MS. notes of Sir William Blizard's lectures, where a case was mentioned, in which the superior thyroid arteries were tied in a case of Bronchocele. So far as related to the expected effect of the

operation, the result was successful, although the patient died three weeks afterwards of the hospital gangrene. Mr. Coates, of the Salisbury Infirmary, has now published the case of Elizabeth Spratt, ætat. 17, in whom the thyroid gland was so much enlarged, as to give rise to great difficulty in deglutition and respiration. The superior thyroid arteries were large, and pulsated strongly. Her general health was good. On the 29th December, a ligature was carried around the superior thyroid vessel of the left side and tied. On the 14th of February of the following year, she was dismissed quite well, the tumor reduced one half in size.

As we shall publish in our next number a review of Dr. Thomson's work on the varioloid disease, we refrain for the present from noticing Sir Gilbert Blane's paper on vaccination.

The paper of Mr. Shaw of Windmill street, which follows next in order, contains some very interesting observations in support of the non-muscularity of the urethra. In reviewing a periodical publication, it will be quite impossible to enter into a full examination of all the facts adduced in support of the author's opinion; were we to do so, each essay would require the same length of time for its consideration, as a volume. We must confine ourselves to the single subject which the author wishes to bring before the profession. Mr. Shaw has discovered, that there is a particular venous structure immediately under the mucous coat of the membranous part of the urethra. These vessels communicate with the veins of the corpus sponginosum. Mr. Shaw conceives that a knowledge of their existence is important, both in an anatomical and chirurgical point of view. In the former, they, in his opinion, go to disprove the existence of muscular fibres in any part of the urethra, and in the latter, are explanatory of the cause of hæmorrhage where the bougie is unskilfully employed.

The dangers of *inversio uteri*, are too well known to require any observation; every case, therefore, which goes to confirm the success of extirpating the inverted uterus, is most interesting to the profession. Mr. Windsor relates, in the present volume, a very valuable history of a case of this kind. The subject of it had the womb inverted, after a delivery, which occurred a year

before the 22d of August, the day on which the operation was performed. Ever afterwards hæmorrhages from the uterine vessels occurred; from these her strength was exhausted, and her life so much endangered, as to justify the performance of the operation, which Mr. Windsor in consultation with Dr. Hull and others then resolved on. A ligature of strong dentist silk, was carried around the uterus as high up as possible, and after it was firmly tied, another contained in a double copper canula was fixed in the indentation which the former had made. It would be tedious to go over the whole history of the after treatment; suffice it to say, that the ligature contained in the canula was tightened daily, the patient suffered much from pain, and was threatened with abdominal inflammation. At the end of twelve days the separation of the tumor was completed. On the 11th of October, she was so well as to be able to walk out, and has since enjoyed good health.

The sufferings which the patient experienced from the employment of the ligature, has induced Mr. Windsor to propose that the extirpation should be accomplished by the knife, either immediately, or a short time after the ligature has been applied. As far as our judgment goes, we would advise either the operation of Mr. Newnham, or the one performed by Mr. Windsor, in the present case, in preference to that of excision; we do not think that the success of the case recorded by Wrisberg, where an ignorant midwife operated, or Mr. Hunter's, of Dumbarton in Scotland, case should be held as warrants for substituting the operation by the knife, one so much more dangerous than the one performed by the ligature.

Until the publication of Mr. Brande's observations in the Philosophical Transactions, it was, from the observations of Fourcroy and other chemists, a generally received opinion, that the urate of ammonia entered as a common ingredient into the composition of urinary calculi. From the observations, however, of that gentleman, and more lately from the remarks of Dr. Marcet, this has been doubted. Dr. Prout has published a paper to prove, that such a substance does occasionally enter into calculi which form in the urinary bladder.

The two specimens examined by Dr. Prout, were taken from

boys who had not reached the age of puberty. Previous to their removal by operation, the patients had suffered the most distressing irritability. This gentleman concludes, but we think without sufficient data, that urate of ammonia only occurs in the calculi of boys who have not reached the age of puberty, and that where they do form, there is an unusual derangement of the health, and great irritation of the bladder produced.

Mr. Dunn mentions a case where a bag of water presented after the delivering of a child, and where there was not a plurality of children; we do not consider this either difficult of explanation or particularly interesting. The acoucheur can never remain long in doubt, as to whether there is or is not more than one child.

Every anatomist knows that the rectum, as it lies in the concavity of the sacrum, forms a sac. Mr. Chevalier has published some interesting observations, to show that the fæces are very apt to be retained there, and from keeping up a continued irritation, tend to destroy that sympathic sensibility which ought to subsist between the actions of this intestine, and those of the colon. A lady who had been affected with cancer in the left breast, became affected with severe pains in the loins, which confined her to bed. She soon afterwards became unable to pass her urine, which required to be drawn off by a catheter. When the state of her bowels were enquired into, her answer was, that the evacuations were small in quantity but frequent, and that this had been her ordinary habit for many years. It was afterwards discovered by chance that the sac of the rectum was much distended, and that from this, its unnatural size, pressure was made on the bladder, which gave rise to the difficulty of evacuating the urine. This case proves, in Mr. Chevalier's opinion, that we may have great constipation, and even a prodigious enlargement in one part of the great gut, yet regular small stools. The necessity of removing the feculent matter so as to restore the natural sensibilities of the intestines, requires not to be insisted on. Our author thinks that this can be best done by the use of glysters; as with them we act only on that portion of the gut where the sensibility is defective. After the common injections have emptied the gut, astringent ones,

formed with a decoction of oak bark or galls may be useful, in promoting the contraction of the sac to its natural size.

The last case in the volume is a valuable one on affections of the meatus auditorious externus, by Mr. Earle. The subject of the case from which his observations are drawn, was a Mr. H. who from his childhood had been liable to occasional attacks of inflammation in his external ear, accompanied with heat, excoriation, and a copious thin discharge from the passage, which affected his hearing more or less for some weeks, but left no considerable deafness behind. Ten months before he applied to Mr. Earle, he had suffered so severe a renewal of the same disease as to be nearly deprived of the sense of hearing.

When the ears were examined, the passages of both sides were much narrowed in their calibres; it was obvious that this was produced by an increased density of the lining cuticle, which had a very white appearance, and was moistened by a thin discharge, which resembled runnet whey, that deposited a substance not unlike small portions of curd. The same white thickened cuticle, expanded as far as the eye could reach. Although the hearing was almost lost, yet a watch applied to the teeth or forehead was so distinctly audible that there could be no doubt but that the functions of the auditory nerves were perfect.

Mr. Earle was led to believe that the deafness arose from the membrana tympani being covered by the cuticle inflected over it from thickening. This idea was strengthened by passing a probe to the bottom of the meatus. The sensation conveyed was not natural, and the patient did not complain of that disagreeable sensation which is always felt when an instrument is brought in contact with the drum of the ear. With the view of destroying the thickened cuticle, a very strong solution of the nitrate argenti, was injected into the meati on both sides, which completely blackened the epidermis. Eschars were formed, and these were washed away by injecting warm water with a syringe. Immediately on their removal from the lower part of the passages, the fluid injected produced a loud sound. His hearing from this time improved, and was in a few days nearly restored.

After the separation of the cuticle, the treatment consisted in applying, night and morning, with a camel-hair pencil, the following unguent:

℞. Hydrarg. nitratis ʒ iv.
cerati, cetacei. ʒ iij.
ol. oliv. ʒ i. M.

This was recommended with the view of stimulating the ceruminous glands to a more healthy secretion; Mr. E. believing that the serous, thin, ichorous fluid discharged from them had operated as the cause of the thickening of the cuticle. Blisters were likewise applied behind the ears, and kept open with the same intention. The author advises, that whenever the waxy matter of the passage is not secreted, that a stimulating local application be employed, which will in all probability check the disease in its commencement.

Memoir and Observations concerning the good effects of the Actual Cautery applied to the Head and Nape, in many Diseases of the Eyes, of the Envelopes of the Cranium, of the Brain, and of the Nervous System. By Louis Valentin, Doctor in Medicine, Ancient Professor, Ex-Physician in Chief of the Armies of St. Domingo, and of the French Hospitals in Virginia, Knight of the Order of the King, and of the Legion of Honour, &c. &c. Nancy, 1815—pp. 176.

(Translated and Abridged by P. S. Townsend, M. D. of New York.)

After remarking in the introduction, that the actual cautery has been in repute down to the present time among the Egyptians, the Arabs, the Chinese (who use it in the form of moxa,) the Japanese, the Greeks, the wandering Tartars, the Indians of North America, (who employ it in the form of ignited spunk, vid. Rush,) the Laplanders, and the Ostiacks of Siberia, (who both employ an agaric of the birch;) he observes, that its utility is also confirmed by the testimony of modern writers, who use it with success upon the chest, in the cure of phthisis where ulceration

has not commenced, in rheumatisms, gout, fungous and carcinomatous excrescences, purulent, cancerous or gangrenous ulcers, anthrax, tetanus, and in hydrophobia, either as a prophylactic or curative remedy.

He says of hydrophobia, that the savages of North America burn out the bitter part by the bark of the white ash, (p. 7–15.)

Dr. Valentin does not enter into the details of these diseases as affected by the actual cautery. He confines himself to its use in diseases of the eyes, and principally in affections of the head, whether internal or external, idiopathic or symptomatic. He has included some observations also on occipital cauterization in yellow fever, as he had witnessed it in the West Indies, and in the typhus of ships. The outlines of this memoir had been previously published by the author in 1708, in the Annals of the Society of Medicine at Paris, and had gained him a gold medal.

M. Valentin uses for a cautery, a flat, thick piece of iron, from the size of a three to that of a six franc piece. In obstinate ophthalmias, and ulcerations of the cornea, causing blindness, and even in amaurosis, he has found it relieve, where all the usual remedies have failed. The iron is heated red hot, and he prefers the eschar to be made on the summit of the head, near the bregma. Its effects are primitive and consecutive. "By the one, it bursts asunder the spasmodic condition, changes the nervous irradiations, and arrests the progress of irritation by a new irritation of greater force; by the other, it draws, as towards a centre, to the most elevated part, the humours of the circumference, from whence afterwards results a discharge more copious, and of longer duration than from a vesicatory."

In the former, its sudden and more powerful operation, excels the moxa, which is often too tedious to bear. The eschar ought to go down to the aponeurosis, and the dressing should be simple oil, or sometimes a suppurative plaster. On the second day, there should be made several crucial incisions on the disorganized tegument, to accelerate suppuration. M. Valentin had an opportunity of verifying the utility of the cautery in his practice at St. Domingo. Diseases of the eye are very prevalent there among the negroes and others, who are accustomed to sleep on marshy and wet places. Where the affection of the eye had been of long standing, he sometimes commenced by vomiting the patient, and on the

same day applied the cautery. The regimen and diet, during the disease, were as usual. Of other remedies than the cautery, he used very few, except the milder collyria. Where the conjunctiva was so engorged as to constitute *chemosis*, or where the transparent cornea was covered with *leucoma*, provided the affection was not recent, M. Valentin had recourse first to scarification of the globe of the eye, and of the inner parts of the lids.*

Case 1st.—M. Erymbat, a French officer at the Cape. He had been despaired of, and was entirely blind. On appearing at the bar of the colonial assembly of the island, in order to procure a pension, it was concluded to refer the case first to the opinion of M. Valentin. His sight was perfectly restored by our author, and the cure is recorded on the journals of the assembly, June 24, 1792. This was a case of chemosis; the cornea was thickened, red, and ulcerated, and the eye-lids much excoriated by the discharge which flowed from the eyes. There were slight traces of transparency however upon the cornea, and the eye possessed a great deal of sensibility. He commenced the cure by a vomit.

Case 2d.—M. Germain, at Toul, in France, 1801. He had had a quartan fever for about nine months, and had been convalescing for six weeks, when he was seized in autumn with violent pains in his limbs, followed shortly after by ophthalmia. The conjunctiva was so engorged, as to form a sort of cushion, and to cover the cornea. He had also with this affection a severe pain in the right parietal bone, augmented on the approach of night. He had tried every remedy. The cautery was applied to the seat of the pain on the parietal bone. In three days the pain subsided, and in five weeks, he was perfectly restored to sight and his usual health. At the close of this case, M. Valentin mentions that he has cauterized the mestoid process in affections of the ear, as otorrhœa, otalgia, otitis, and with success. He proposes this cautery also in surdity of the organ, in tic doloureux, and in the suffocating paroxysms of croup; and has found it succeed more fre-

* Scarification of the globe itself ought, in all instances, to be abandoned. You cannot take off a sufficient quantity of blood from the part, while the inner parts of the lids, particularly those of the lower lids, always afford an abundant source. I have seen this principle fairly tried in the practice of the New York Dispensary. TRANSLATOR.

quently in cephalalgia than the seton, scarifications to the scalp, or arteriotomy. It is said, that Hippocrates used to apply the cautery in affections of the head, to eight different parts. A case is also alluded to by M. Valentin, where it cured a suppression of the catamenia. In the malignant fever of the West Indies, and in the typhus of ships and hospitals, where convulsions, furious delirium, and horror of drinks and of medicines existed, he has found the application of the incandescent cautery to the occiput and nape, of great service. So where, in these diseases, there was great prostration and coma, but not where effusion of the brain had commenced.

Case 3d.—Mademoiselle Pays, at Nancy, 1786, ætat. 48, seized with frightful convulsions, delirium, horror of drinks, &c. was instantly relieved by occipital cauterization; but the fever went on, and assumed a putrid type, from which she recovered in fifteen days.

Case 4th.—M. Verron, at the Cape, 1791, ætat. 20.—Laboured under a remittent, which, on the 10th day, assumed a malignant character, with extreme prostration, vomiting, tympanitic condition of the abdomen, cold extremities, insensible pulse, yellowness of skin, &c.—Cauterized the occiput; recovored on the 30th day.

Case 5th.—M. Arnould, a watchmaker at the Cape, 1792. Similar to the last, the malignant symptoms coming on on the 14th day with convulsions, and ending in an asphyxia, in which state he was pronounced by the physician and neighbours to be dead. On this day, which was the 16th, the occipital cauterization was tried to the extent of three inches. He immediately awakened from his insensibility, and was well on the 22d day from the commencement of the attack.

Case 6th.—Madame Villaret, at Fort Royal, Martinique, 1802. This case was related to M. Valentin by another physician. The patient, a few days after her arrival, was attacked with a nervous fever, and on the 15th day of the disease, was deprived of sense and motion. The moxa was applied, and sensibility restored; but having been continued too long, exfoliation of the superior border of the portal bone took place, and continued for a year.

Case 7th.—M. Ottaviani, a Sicilian merchant, of Marseilles, 1809, ætat. 44, strong constitution, and of a high stature, attacked

with an ataxic fever, with frantic delirium, occasionally furious convulsions, tetanic spasms, and dread of water. The red hot iron was applied to the occiput and nape, with cold shower-baths to the head. A subsidence of the disease took place, and a fistulous opening made its appearance at the verge of the anus. The delirium still occasionally supervened. In the fifth week he was operated upon for the fistula; but having prematurely indulged in coition, there were some appearances of mental alienation. These disappeared; but the mania being brought on the next year, in consequence of commercial misfortunes, he was sent to the hospital, and died of adynamic fever.

M. Valentin found the occipital cauterization of great benefit in an epidemic typhus, which attacked a French squadron at Hampton Roads, in Chesapeake bay, in 1794. In these cases he accompanied it with cold affusion. Corroborative of the good effects of the cautery, he mentions that intermittents have been cured by the accidental burning of some part of the body or extremities. He calls this remedy the *ultima ratio medicorum*, and says it ought to be used where other means have failed. He relates three cases of ataxic fever where it was unavailing. The symptoms in these cases were delirium, convulsions, trismus, and difficult deglutition, feeble pulse, general sweating, &c. In the hospital for the sick of the allied armies, established in Lorraine, he found the Russians much less subject to typhus than others. In one case here of typhus, the cautery failed. He claims the honour of having used cold affusions before Currie of Liverpool. He employed them at Nancy in 1784. He has inserted a case of acute hydrocephalus in a child, cured by Dr. Tracy, with the moxa, and another of cerebral fever by the same.

Case 15*th*.—A negro of St. Domingo, cured by M. Valentin at Norfolk in 1794, of mania by the cautery. He was about 25.

Case 16*th*.—A French surgeon at Norfolk, 1795, also cured of recent mania, by this remedy, accompanied with shower-baths to the head. This was a violent case; the symptoms were, melancholy, raving, hæmoptysis at one period, and many other distressing symptoms, usual in the severe forms of this disease.

Case 17*th*. A Frenchman at Norfolk, 1795, recent mania. Bleeding, vomiting, &c. having failed to relieve him, the cautery

was applied, and he was cured. Cold shower-baths to the head also used.

Case 18*th*. A little girl, at Norfolk, lost her reason at five years, and became dumb. Dr. Valentin relates this as a solitary case, Pinel himself never having seen mania before puberty.* Six years afterwards Dr. Valentin treated the case with the cautery. She grew better, but, having removed to the country, he had not an opportunity of learning the result.

Case 19*th*. A young lady, at the Hospital of St. François, in France, 1813, maniacal from eight to nine months; menses became suppressed in the commencement of the disease; but seven months after they returned, and having continued for two months without alleviating the alienation, she was cauterized, and subjected to the shower-bath and occasional vomiting. The cure was effected in a short time.

M. Valentin then makes some therapeutical observations on mental alienation. He relies, it appears, chiefly upon the cautery and vomiting. He quotes Dr. Fischer, physician of the civil hospital of Strasburgh, in favour of the use of tartarized antimony. Dr. Fischer depended more on this than any other remedy. He mingled it with the drink and food of the patient, from the quantity of 14 to 20 gr. every day, and without producing any sensible evacuation. M. Pinel seemed to approve of the cautery, in a conversation with M. Valentin, but was afraid the introduction of it into the Salpétrière might bring on dangerous terror among the females employed in the care of the maniacs. M. Valentin thinks the cautery would have been especially useful to the persons described by Haslam at Bedlam, as it was found by Haslam that the teguments on the posterior part of the head were particularly relaxed and wrinkled, and that in the greatest number of them there was serous effusion between the dura mater and arachnoid membrane, or in the lateral ventricles. M. Valentin speaks of Rush's work on the diseases of the mind, published at Philadelphia, 1812. He remarks, in a note, that he

* This at first would seem to be an extraordinary and unexampled period of life for the invasion of mania. It may be noted here, however, in confirmation, that a case of melancholy in a boy of eleven years of age, ending in suicide, occurred a very few weeks since at Charleston, S. C. TRANSLATOR.

would have translated it for its curious and interesting facts, were it not that we already had the good work of Pinel, and were in expectation of that of Dr. Esquirol. Rush recommends salivation, and it has been adopted, says M. Valentin, by some German physicians. M. Valentin remarks, that corpulency generally supervenes in convalescence from mania, as happened in the last case above related, and as is mentioned also by Esquirol.

Case 20th. Epilepsy in a girl ætat 17, 1809. Paroxysms returned every month, between the 17th and 20th day, succeeded by estrangement for some days. The menstrual evacuation did not appear to modify the disease. Cured by the moxa applied to the breast and nape, by Dr. Mercurin, in his establishment at St. Paul, France.

The failure of De Haen and Pouteau in the use of ustion to the denuded cranium, in gutta serena by the former, and in epilepsy by the latter, is to be attributed, he thinks, to the improper application of this remedy. The previous incision of the soft parts, for instance, was quite useless. He justifies cauterization of the head in epilepsy, on the ground that this disease has often been cured by wounds or burns accidentally made to this part.

Percy relates, on the authority of Fabricius and Mercurialis, that it was the practice at Florence and Padua, to cauterise the occiput of new born children, to preserve them from, or to cure them of epilepsy and comatous affections, to which they are subject in that climate. So much was this practice in repute at the time of Dominicus Panaroli (who was opposed to it,) that it would have been difficult to have found at Florence, ten infants who had not been thus cauterised. Panaroli died in 1657; and since his time, the practice has totally disappeared. Hippocrates had observed that ulcers on the head and about the ears, cured epilepsy. M. Valentin refers to a case cured by ustion, where the bone was not involved, and another in which trephining for an accidental fracture, had cured the patient of an epilepsy, under which he had been labouring for many years.* Another successful case of this

* Professor Mott, of the university of New York, relates the case of a man in the western part of the state of New York, who laboured for a long time under a severe cephalalgia, and having desired that the trephine might be used to his head, it was accordingly applied, a piece taken out, and the cure effected. TRANSLATOR.

kind is related, where the trephine was applied to the angle of the lamboidal suture.

M. Valentin does not think adustion would be of any service, where there exists any idiopathic organic derangement, as a tumor, a concretion in the brain or upon its envelopes, a fungus or foreign body in this organ, &c. Where epilepsy does not proceed from such causes, and where the aura epileptica does not set out from any fixed point, M. Valentin proposes to alternate the application of the cautery upon different parts, of the sternum, nape, hypochondrium, head, epigastrium, extremities, &c. He refers to a case at Lyons, in 1811, of a girl attacked with epilepsy from fear, and cured by moxa applied to the inner part of the right thigh.

M. Valentin has had no opportunity of trying the cautery upon the epileptics, in his own practice. Upon two patients, he made an unsuccessful attempt, but burnt only the outer integuments, so that no good effect was produced.

Case 21st. Mr. Charles Bennet, a merchant at Alexandria, Virginia, ætat. 25, 1797, of an excellent constitution, about four months before, had fallen into a cellar, and injured the right temple and parietal bone. On recovering from the contusion, an excruciating pain in the part continued there with vertigo, and rendered him miserable, night and day. The patient having first taken a vomit, the cautery was applied to the part, and the cure immediately effected.

Case 22d. Thomas Fist, an Englishman, barber, near Norfolk, ætat. 28, 1797, had been afflicted for 20 years with a violent pain on the right side of the head (cephalea hemicrania.) It had never intermitted more than six weeks at a time. He experienced a slight abatement in the pain at night, and an exacerbation on the rising of the sun. His mother, he said, had had the same affection. Every remedy had been used. The cautery was applied, and a radical cure immediately effected.

Case 23d. A female cook, ætat. 61, at Nancy—for 10 years afflicted with severe pains in the head, which M. Valentin calls céphalalgie. In 1814, when she consulted him, he pronounces it *céphalée*, which was so intense, that she had not slept for the last three months and a half, accompanied with an asthma. The pain

being more severe at the summit than the circumference, she was cauterized on the former, and in one year was well.

Case 24th. Madame Sonrier, at Nancy, 1814—ætat. 53. Seventeen years before, began to feel severe pains in the head, interrupted at times, for three or four years, by an eruptive disease upon the epigastrium and extremities. For the last three months attacked with hemiplegia, followed by an apoplexy, when the eruption disappearing suddenly, there came on long and violent paroxysms of rheumatismal *cephalalgia.* The pain was greater towards the sinciput. All remedies having failed to relieve the hemiplegia and cephalalgia, the moxa was applied, by which the latter affection was removed.

In note 12, at the end of the volume, is related the case of a man who died in 1781, near Nancy. For three years afflicted with violent céphalée—fell down at the expiration of this period, as if struck with apoplexy, and was laid out to be buried. He recovered, and for four years was perfectly well; at length he fell down dead. On opening the body, the cerebrum was completely suppurated, and only a small part left, of the size of an egg.

Revue Médicale, Historique, et Philosophique. Par MM. Bally, Bellanger, F. Bérard, Bestieu, Bousquet, Delpech, Desportes, Double, Dunal, Esquirol, Gasc, Giraudy, Jadeoux, Laurent, Nicod, Prunelle, Rouzet. 1re année (1820), 1re et 2e livraisons. Paris.

The initial article of this journal is a paper, drawn up by Mr. Prunelle, upon the influence of medicine on population. As this article, however, has no immediate relation to the primary objects of our journal, we shall not stop to give an analysis of it, but pass on to the second, which is more particularly interesting to the profession, and of which, on account of the novelty and importance of its matter, we propose to give a full account.

The article consists of a very circumstantial analysis of a work recently published at Paris, entitled *De l'Auscultation Médiate,*

ou traité du diagnostic des maladies des poumons et du cœur, fondé principalement sur ce nouveau moyen d'exploration. Par R. T. H. Lænnec, D. M. P. Médecin de l'Hôpital Necker, Membre de la Société de la Faculté de Médecine de Paris, &c. &c. 2 vol. 8vo. 1819.

Médiate Auscultation, or a treatise on the diagnostics of the diseases of the lungs, and of the heart, founded principally upon this new mode of exploration, &c. &c.

Avenbrugger, a German physician, was the first who employed percussion of the thorax as a diagnostic for diseases of the chest. It soon, however, sunk into neglect, and was afterwards again brought to the notice of the profession by Corvisart. Convinced of the importance of the discovery of Avenbrugger, but seeing, at the same time, its numerous imperfections, Mr. Lænnec was led to look for a mode of exploring the thorax, that might afford results more precise and obvious.

In 1816 Mr. Lænnec was consulted by a young lady labouring under symptoms of disease of the heart; percussion afforded no satisfactory results, on account of obesity. The age and sex of the patient prevented him from applying his naked ear to the region of the heart, whilst using percussion, a mode of examining which he had been in the habit of using. Recollecting the fact of the ease with which sound is transmitted through solid bodies, he rolled up a quire of paper, into the form of a solid cylinder, placed one extremity of it upon the præcordia, and applied his naked ear to the other. He now heard the pulsations of the heart, much more distinctly, than he had ever done before, in any case, whilst applying his ear immediately to the thorax. Presuming, from this experiment, that this mode of examining the thorax might become an important means, not only for the investigation of diseases of the heart, but also form a valuable diagnostic for other diseases of the chest, he entered on a series of experiments on this subject, in the Necker Hospital. The results of these trials fully realized the expectations he had formed, and convinced him, that this new mode of exploring the chest, is calculated to render the diagnosis of most thoracic diseases more certain and circumstantial, per-

haps, than the surgical diagnostic of the sound itself, or the introduction of the finger.

The instrument which he finally adopted, as the one best calculated for this mode of examination, is a cylinder of wood, a foot in length, perforated lengthwise through its centre with a tube three lines in diameter, and made to fold in the middle, by means of a hinge, to render it more portable. One extremity of the tube is cut out, in the shape of a funnel, of about one inch and a half in depth. The instrument, thus disposed, is best suited for the exploration of the sounds arising from respiration. For the exploration of the voice and the pulsations of the heart, the cylinder is to be converted into a simple tube of thick sides, by inserting into the funnel-shaped extremity a piece of wood, exactly fitted to the cavity, and fastened to the tube of the cylinder by means of a small brass tube passing through it, and entering some distance into the tube of the cylinder. The end intended to be applied to the breast of the patient is to be slightly concave. This instrument our author calls *stethoscope*.

Mr. Lænnec's work is divided into four parts. The first treats of the manner of exploring the voice; 2d, of the exploration of the respiration; 3d, of the exploration of rattling (*râle*) in the chest; and 4th, of the exploration of the circulation.

Chap. I. Of the exploration of the Voice in general.

When a healthy man speaks or sings, his voice reverberates within the lungs, and produces, through the whole extent of the sides of the thorax, a kind of trembling, which may easily be felt by applying the hand to the chest; but this does not take place, when, in consequence of disease of the lungs, this viscus has become impermeable to the air, or when a fluid is effused between it and the sides of the thorax. This mode of examining with the hand is, however, of little value, since a variety of causes render it wholly nugatory; such as fatness, infiltration under the integuments of the thorax, &c. Explored with the cylinder, the phenomenon just mentioned is very distinct. Different parts of the thorax are found to present differences in the intensity of this phenomenon. Thus in the axillæ, on the back,

between the internal edges of the scapulæ and the vertebral column, and about the angle formed by union of the sternum and clavicle, this reverberation of sound is much more distinctly noticed than at other parts. If the cylinder be applied to any of these parts, the voice is much stronger, and seems to be brought close to the ear.

Being occupied with experiments of this kind, Mr. Lænnec examined the thorax of a woman, affected with slight bilious fever and a recent cough, which appeared to be the effect of a pulmonary catarrh, when, on applying his cylinder under the middle of the right clavicle, he was struck with an extraordinary phenomenon, which he had never before noticed. Causing the woman to speak, her voice seemed to issue directly from the breast, and to pass entirely through the hollow cylinder. This phenomenon took place only when the cylinder was applied to a space of the thorax about one inch square. Similar experiments, made about the same time in the Necker Hospital, afforded similar results. Mr. Lænnec thinks that this phenomenon might arise from the reverberation of the voice along the sides of tortuous canals, produced by the suppuration of tubercles, and known under the name of ulcers of the lungs.

Chap. II. Phthisis Pulmonalis.

According to Mr. Lænnec, tubercles are the only causes capable of producing phthisis. Mr. Bayle's researches have taught us more exactly the development and progress of tubercles, and the general effects which they produce upon the animal economy. He was well aware that they are not the immediate results of a previous inflammation of the parenchyma of the lungs, and that the small homogeneous, greyish, or yellowish pale bodies, which constitute the tuberculous mass, are not formed at the expense of the pulmonary tissue, and that tubercles, when they are found in the form of isolated bodies, develope themselves by the accidental formation of a new and morbid tissue within the parenchyma of the lungs. Pathological anatomy has, indeed, demonstrated, that this is the manner in which the tuberculous tissue, as well as all

accidental tissues, which bear no analogy to the primitive tissues of the economy, are developed.

Mr. Bayle has demonstrated the existence of false membranes, lining the sides of abscesses formed by suppurated tubercles. Mr. Lænnec has shewn that there is also an imperfect cartilaginous membrane formed behind the inner membrane of Mr. Bayle. He states that while the cartilaginous membrane is forming, the inner one loses its attachments with the pulmonary tissue, separates in pieces, and is expectorated in shreds with the matter formed in the tubercles.

When tubercles suppurate, they open into the bronchiæ: these openings being narrower than the cavities with which they communicate, both necessarily remain in a fistulous state. These cavities are frequently surrounded by tubercles of various sizes, which, entering into suppuration, form communications with the principal cavity. Hence tortuous sinuses are formed, which by degrees extend through the pulmonary substance. Bands of condensed pulmonary tissue, infiltrated for the most part by tuberculous matter, cross these excavations. Mr. Bayle mistook these bands for blood-vessels, but Mr. Lænnec affirms that they are not so. He thinks, also, that the greater part of the expectoration of phthisical patients is the product of secretion from the bronchiæ. "I may observe," says he, "that when the cavities of suppurated tubercles are lined by the false membrane described above, they are often entirely empty; if they do contain any purulent matter, it resembles much less the expectoration of a phthisical patient than that which is found in the bronchiæ."

Mr. Lænnec observes, that when tubercles of the lungs are very numerous, even though very small in size, they sometimes occasion death before any of them enter into suppuration. When they are few in number, they are very often all found in a state of ulceration and excavation. Most frequently, however, they are found in different states of development in the same lung.

The French physicians seem to be generally aware that tubercles are but very rarely confined entirely to the lungs in consumptive patients. These bodies are simultaneously developed in various organs, as has been conclusively demonstrated by the

researches of modern pathologists. Mr. Lænnec, who has dissected more than two hundred phthisical subjects, has pointed out the order of frequency in which tubercles are found in the different organs. It is as follows: the bronchial and mediastinal glands; the glands of the neck; the mesenteric glands; those of all other parts of the body; the liver, the prostate, the peritoneal surface and the pleura, the mucous membranes, the epidermis, the different ducts, the testicles, the spleen, the heart, the uterus, the brain, the cerebellum, the back of the neck, the vertebral column, &c.

The development of pulmonary tubercles appears frequently to be a consequence of an alteration of a similar character in some other organ of the body. Frequently, too, the pulmonary affection remains entirely latent during the greatest activity of a glandular or scrophulous affection in some other part of the system, but commences and advances with frightful rapidity, when, by the efforts of nature or art, the primary disorder has been relieved.*

When the tubercles have suppurated and formed excavations, the examination of the voice, by means of the cylinder, affords phenomena, to which the author gives the name of *pectoriloquism*.

In pectoriloquism the voice is generally somewhat more acute than when heard by the naked ear, and has a choked or muffled sound, analogous to that of ventriloquists. Sometimes it seems as if the patient were speaking into one's ear, and with so strong a voice, tha it pains the head. In some patients, the voice heard through the cylinder, sounds as if passing through a brass tube. This phenomenon, which is attended by a singular vibration, is called *egophony*.

Very rarely there is a ringing sound heard in the voice, similar to that of a bell, or a glass, when it is about ceasing to ring. This ringing resounds in the tube, after each word the patient pronounces. Mr. L. calls it *metallic* tingling.

Pectoriloquism is most distinct, when the parietes of the abscess are thin. It is also more perfect in proportion as the cavi-

* See an interesting essay upon the connection of external glandular swellings and pulmonary consumption, by Dr. J. Parrish, published in the second volume of the Eclectic Repertory.

ties are less tortuous. Cavities that are much longer in one direction than in another, or that are flattened by having their sides pressed together, are the least fit to produce the effects of petoriloquism.

When a number of excavations, communicating with each other, form large and various sinuosities, the voice always seems to pass directly through the cylinder, but the articulation of the words is more confused and smothered.

When abscesses of the lungs contain matter of the consistence of pus, pectoriloquism is attended with a kind of gurgling noise, (*gargouilment*,) rendering the articulation of the words less distinct, and accompanied by a rattling sound.

If pectoriloquism be continued or constant, and unattended by any strange sound; and if there be no rattling noise heard in the breast, it may be concluded, that the cavity of the abscess is altogether empty.

Mr. L. then goes on to make some interesting observations on the possibility of the cure of phthisis, and on its mode of treatment. The opinion, that genuine tuberculous consumption is incurable, is not, according to our author, well founded. Ten cases reported by him, and he assures us that he might report many more, seem to demonstrate, that pulmonary tubercles are not, in all cases, necessarily fatal. The cure of them, after the tubercles have suppurated, may take place in two ways:—1st, Either by the conversion of the ulcer into a fistula, having its sides lined, like all ulcers that may exist without implicating the general health, by a membrane; or, by a cicatrix, more or less perfect, of a cellulo-cartilaginous texture, formed by the approach and adhesion to each other, of the opposite sides of the cavity. In some cases, nature has been taken, as it were, in the fact, labouring to effect the mutual adhesion of the sides of the cavity. Mr. L. observed this very distinctly in the fifth case which he reports, where the adhesion was incomplete, in consequence of the presence of a small portion of tuberculous matter, which had not yet suppurated.

The general symptoms of phthisis bear no constant relation to the extent of the organic disorder in the lungs, and to the different stages of its development and progress. Fever, emaciation, and

watchfulness, are generally proportionate to the sensibility of the patient. These symptoms often acquire a very considerable degree of violence, whilst the organic lesion of the lungs has, as yet, made but little progress; and, on the other hand, tubercles are often in a state of complete suppuration, whilst the emaciation, and general disturbance of the system, are comparatively trifling. The character of the *expectoration*, however, always affords a certain indication of the organic condition of the lungs.

Abscesses very rarely form in the pulmonary tissue, in consequence of inflammation. *Vomicæ*, which physicians consider as collections of puss formed in the proper substance of the lungs, from inflammation, are, in fact, the result only of the softening (*ramollissement*) of large tuberculous masses.

Chap. III. Of the Dilatation of the Bronchiæ.

Mr. Lænnec is the first pathologist, who has described the organic alteration in question. This affection is not very uncommon, and has hitherto remained unnoticed, in consequence of the dilated branches, having been mistaken for the larger ones.

This dilatation of the bronchiæ, is found only in subjects who have died of chronic catarrh. It is sometimes so great, that branches, which in the natural state, would have hardly received a fine probe, possess a diameter equal to that of a goose quill, or even to that of the finger. The extremities of the bronchial tubes, thus dilated, terminate in *cul-de-sacs*, or cells of the size of a hemp seed, a cherry stone, a filbert, and even an almond. The internal membrane of the dilated bronchial branches, is reddish, and evidently thickened. This organic lesion is more apt to occur in the superior, than in the inferior parts of the lungs. It is seldom visible in the common trunk of the bronchiæ, even when, at the same time, its ramifications are so much dilated, that some of them become equal in size to the common trunk.

When the dilatation is confined to one or two branches, an habitual cough, commonly weak, not very frequent, and attended by a slight mucous expectoration, is the result. If a great number of the bronchial branches are in a dilated state, a chronic catarrh

exists, which lasts as long as the life of the patient, without, however, appearing to influence its duration.

Chap. IV. Egophony.

Mr. L. confounded this, for a long time, with pectoriloquism. It resembles pectoriloquism in affording a strong sound of the voice on using the cylinder. But the voice seems very rarely to enter the tube of the cylinder, or to pass entirely through it, as in perfect pectoriloquism. In *egophony*, the voice is more acute and harsher than in the natural state, and produces an illusion, as if some one were speaking in the patient's breast. The voice has a trembling, *argentine* sound. Mr. L. considers egophony as a pathognomonic symptom of effusion within the pleura. This sound resembles the effect which is produced by a small piece of coin placed between the teeth and the lips of a man speaking, or the voice transmitted through a cracked reed, &c.

Egophony is always heard in a considerable extent of the thorax, and not simply in one particular spot, like pectoriloquism. It is generally heard in the space comprehended between the internal edges of the scapulæ and the vertebral column, along the edge of the inferior angle of this bone, and in a zone of about three fingers breadth, which extends in the direction of the ribs, from the middle of the scapulæ to the sternum.

Egophony arises from the natural sound of the voice in the bronchial branches, being transmitted through a thin layer of a fluid effused between the lungs and the pleura. He considers it, therefore, as a pathognomonic symptom of effusion into the cavity of the chest. Egophony is always heard on the second or third day of a pleurisy; when the effusion becomes more abundant, it ceases to be heard. A change of position of the patient, produces some variation in the place where egophony is heard.

Part II. Of the Exploration of the Respiration in general.

For the exploration of the respiration, the cylinder cut out at one extremity in the form of a funnel, is to be used. If this instrument be applied to the breast of a healthy man, a light, but quite distinct murmur is heard in the lungs, during respiration.

This murmuring sound is most distinctly noticed, when the cylinder is applied in the hollow of the axilla, and on the space comprised between the clavicle and the superior edge of the trapezium muscle.

If the respiration be heard very distinctly, and with equal strength in almost every part of the breast, we may be certain that there is no effusion within the pleura, nor any congestion whatever in the lungs. When, on the contrary, the respiration is not heard within a certain extent of the breast, we may be assured that this part has become impenetrable to the air by a morbid cause.

Peripneumony.—Peripneumony is divided into three degrees, founded upon anatomico-pathological distinctions. In the first degree, the lungs are heavier and firmer than in their natural state. Its external colour is livid or violaceous; the substance infiltrated with a bloody serosity, and less crepitating than in a healthy state. Its spongy texture is still quite evident. This state, is the engorgement of the lungs described by Mr. Bayle.

In the second degree, the lungs are no longer crepitating; they possess the colour, density, and weight, of the liver; hence, it is called *hepatization.* Internally, the lungs present a more or less deep redness, marked, in a very remarkable manner, by specks, formed of the black pulmonary matter, the bronchial branches, the blood-vessels, and the membranous partitions that divide the substance of the lungs into unequal portions. When cut into, and held to the light, they show no cellular structure, but a surface of a granulated appearance.

In the third degree, the pulmonary tissue, preserving its firmness and granulated appearance, has a yellowish colour, and exudes a purulent matter from the surface of incisions.

The most common seat of peripneumonic inflammation, is the inferior part of the lungs; from this part it gradually extends to the other parts of this organ. Tubercles, on the contrary, generally commence in the superior parts of the lungs.

The general diagnostics laid down by pathologists for this disease, are not, in the opinion of Mr. L., as certain as they are com-

monly supposed to be. The expectoration, if it be sufficiently copious, is the only certain pathognomonic sign of this disease. In peripneumony, and in no other disease, the expectoration is a white, slightly yellow, or greenish substance, a little semitransparent, mixed with globules of air, and very viscid. Percussion of the thorax, after the method of Mr. Avenbrugger, is a much more certain means for ascertaining the existence of peripneumony, throughout all its stages. But even this method is insufficient in some cases. The exploration of the respiration by means of the cylinder, has many advantages over the method of Avenbrugger.

In the first degree of peripneumony, the respiration is still audible in the affected part, but it is less strong and sonorous than in the other parts of the breast. It is also accompanied, particularly on inspiration, with a rattling noise, resembling the crepitation of salt, when thrown on hot coals.

The second and third degrees are known by the total absence of the murmur in the respiration.

The respiration, in the first and second degrees of this disease, when the cylinder is applied to the healthy parts of the lungs, is attended with a sound resembling the voice of an infant.

When, in an inflammation of the lungs, resolution commences, the respiration, heard through the cylinder, is attended with a murmuring sound. This phenomenon, which takes place at first, only at the superior parts of the affected place, gains gradually in extent until the resolution is perfected.

In the third chapter, Mr. L. describes, what he calls gangrene of the lungs, which is an extremely rare affection. The *gangrene* is *circumscribed* or *uncircumscribed*. The latter differs very considerably from the former, both as to its effects, and its anatomical character. It occupies the greater part of a lobe, and frequently, almost an entire lung. The pulmonary tissue of the affected part is more humid, more easily torn, than in the natural state; its colour presents various shades, from a dirty white and slightly greenish, to a deep green, and almost black hue, mixed sometimes with a brown, or a yellowish brown shade. Some parts readily fall into a putrid solution. A fluid clouded with a greyish green, and possessing a gangrenous fetor, exudes from

the affected parts. There is a total prostration of strength in this disease. The pulse is small, depressed, and frequent; the cough rather frequent than strong; the expectoration liquid, of a very remarkable green colour, and gangrenous fetor. The oppression is extreme, and the patient dies, suffocated by an effusion into the lungs.

The circumbscribed gangrene occupies but a small space of the lungs. It exhibits irregular eschars of variable sizes, of a black, verging to a green, more humid, more compact and hard, than the natural texture of the lungs.

These eschars resemble very much those which are formed by caustic potash, when applied to the skin.

The abscesses formed by gangrene, constitute, according to Mr. L. the ulcerous phthisis of Mr. Bayle.

Chap. III. Of Emphysema of the Lungs.

Mr. Lænnec is the first who has given a full and general description of this disease. It consists in a morbid dilatation of the bronchial vesicles or air cells; the size to which they are distended in this disease, varies from that of a millet seed, to that of a middle-sized bean.

The most dilated vesicles do not, commonly, project beyond the surface of the lungs; sometimes, however, they slightly project; sometimes, though still more rarely, they are altogether salient and globular, and cannot be moved from their place by pressure. They are perforated at the bottom by small holes, which establish communications with the neighbouring cells and bronchial branches.

When the disease remains in this state, the air is, as yet, contained in its proper vessels; but when the distention becomes too considerable, the air cells give way in some points, and the air is infiltrated into the surrounding cellular tissue. Irregular vesicles are then observed on the surface of this organ, of different sizes, from a hemp seed to an egg. These are moveable, and readily pressed away with the finger. Finally, the excessive distention of the bronchial cells, may occasion the rupture of the pulmonary tissue. These cavities, filled with air, frequently also

contain some blood. The branches of the bronchiæ are very rarely dilated in emphysema; a circumstance which is remarkable, in as much as the cause which dilates the air vesicles, must act equally upon the bronchiæ.

When one lung only is affected, it is much more voluminous than the other. In this disease, there is an habitual difficulty of respiration, which is apt to have irregular exacerbations. When the disease has reached a certain degree of intensity, the skin presents commonly a dull and earthy like aspect, with a slight violet tinge in certain points; the lips are purple and swelled.

The pathognomonic character of emphysema of the lungs is derived from a comparison of the signs given by percussion with those furnished by the *mediate auscultation*. The respiration by means of the cylinder is hardly audible in the greater part of the breast; although this cavity afford a clear and distinct sound by percussion; the part, also, where at first, the respiration is scarcely audible, becomes in a few hours the part where it is most distinctly heard, whilst in parts, where it was at first quite audible, it has ceased to give sound. A slight hissing noise is also occasionally heard in the part corresponding to the affected spot.

Chap. IV. This chapter is devoted to a consideration of the accidental productions developed in the lungs. As we have already taken up so much room with this analysis, and as what is said in this chapter, does not contribute to develop any peculiar opinions of the author, we will pass on to notice the remaining articles of this journal.

The second article consists of a review of *Brodie's work on the diseases of the joints.*

The reviewer speaks of this work in high terms of praise. Its merits are, however, by this time, so well known to the profession generally, that it were useless, perhaps, to notice this article particularly in this place. The reviewer makes an observation, which is worthy of notice. He says, "As for the remainder of the first six cases, reported by Mr. Brodie, they throw much light upon the theory of gonorrhœal metastasis with which rheumatic affections are too often confounded." We consider this observation of much value. We have a case under our own notice at present

in which the symptoms of rheumatism and gonorrhœal discharge, alternate.

The next article which we will notice is,

A memoir on Fistula Lachrymalis, by M. Nicod, principal surgeon to the hospital of Beaujon, and surgeon to the king.

The author thinks that the treatment of this disorder is still susceptible of much improvement; and this he maintains is only to be brought about by uniting therapeutical treatment with mechanical surgery in the management of this disease.

Scarpa, in his excellent treatise on the diseases of the eye, does not view fistula lachrymalis as the last stage of lachrymal tumor. He has demonstrated, that *fistula* is the third stage of an affection, which had been but imperfectly noticed before him; that the distention of the lachrymal sac, is but the *second* stage of this affection, and that this affection, which he denominates *puriform palpebral discharge*, constitutes the *first* stage. This *puriform palpebral discharge*, arises from chronic inflammation of meibomious glands. In consequence of this inflammation, they secrete a superabundant quantity of the greasy matter, which is peculiar to them, and which, passing through the lachrymal ducts, and mixing with the viscid matter furnished by the inner membrane of the inferior eye-lid, descends, only in part, and with much difficulty, into the nose.

Contrary to the opinion of the majority of surgeons, Scarpa does not ascribe the cause of *tumor* and fistula lachrymalis, to a contraction or obstruction of the nasal duct. He alleges, *that these disorders depend on a peculiar state of the eye-lids, and that the nasal canal is very rarely obstructed.* This opinion he founds on the facility with which, in almost all operations for this disorder, a canula, or sound, is introduced into the nasal canal; and particularly on the efficacy of a treatment, applied solely to the eye-lids.

Scarpa, however, thought it impossible to perform a cure, when there is tumor of the lachrymal sac, *without an operation.* In such cases therefore, he advises opening the tumor, not, indeed, with view of introducing a stilet or canula into the nasal duct, but for the purpose solely of introducing into the lachrymal sac, an oint-

ment which he employed in chronic ophthalmia,and which he supposed acted by *re-establishing the elasticity of the sac.* The author of the present memoir, approbating in part this treatment of Scarpa, objects to the inferences which he draws from it. He thinks, and we conceive with much justice, that there is really a contraction of the nasal canal, produced by a chronic inflammation of its internal membrane, and that the ointment introduced into the sac relieves the disorder by curing the inflammation, and consequently the thickening of this membrane of the canal and sac, just as the same ointment, or other collyria cure the inflamed meibomious glands, or the conjunctiva of the eye-lids.

A case which the writer of this memoir had in 1807 led him to modify the treatment proposed by Scarpa, and to undertake the cure of lachrymal tumor *without any operation whatever.* This case was a very obstinate chronic ophthalmia. He introduced under the eye-lids some of *Janin's* ointment, by which the patient got better. After having used the ointment for some days, the patient said, *that he thought he noticed the colour of the ointment in the mucus discharged from the nose.* In 1808 he treated another case of *palpebral flux.* And in 1809, a third case came under notice, which he also cured. In both these cases, the anti-ophthalmic ointment was plainly distinguished, mixed with the mucous discharge from the nose. He gives an account of five more cases, all of which go to prove, in the most incontestible manner, that fistula lachrymalis may be cured without any operation.

It appears, therefore, from the experience of our author, that tumor and fistula lachrymalis depend on chronic inflammation of the internal membrane of the sac and nasal canal; that the nasal duct, contrary to the opinion of Scarpa, does become obstructed, in consequence of the thickening of the sides of its internal membrane, from chronic inflammation; and that, as this obstruction depends upon chronic inflammation of the inner membrane, it is to be removed by such remedies as are found to relieve chronic inflammation of the conjunctiva. With a view to apply the remedy immediately to the inner surface of the sac, Scarpa opened it, and introduced into it Janin's anti-ophthalmic ointment. The author of this memoir affirms that this operation is unneces-

sary, since the ointment, when simply introduced under the eyelids, as in ophthalmia, will readily find its way into the sac, through the punctæ lachrymalis. He avers, that he has been uniformly successful in the cure of lachrymal tumor by this simple mode of treatment.

The following is the ointment which he recommends:

℞ Axung. ℥ij, oxide zinci, bol. armen. ammoniated muriat, merc. āā gr. xij. Misce.

The concluding article of this number is entitled, "*Researches on the situation of the internal orifice of Fistula in Ano, &c.* By F. Ribes, M. D.

The author of this memoir states, that he has for above twenty-five years directed his attention to the investigation of fistula in ano. He examined a very great number of dead bodies that had fistula in ano. In these examinations, his first object was, to ascertain the relative situation and condition of the internal orifice; and then, to examine the course which the fistulæ took, and the parts through which they passed.

He found, in the greater number of cases, the internal orifice situated immediately above the place where the internal membrane of the rectum unites with the external skin. It never opened more than five or six lines above this place.

The tract of the fistula was found to vary. It generally, however, descended between the mucous membrane of the rectum and the sphincter ani, until it reached the inferior part of this muscle, then changed its course, and took an oblique direction between the external sphincter and the skin, and finally opened in the vicinity of the margin of the anus.

In a good many cases, the fistula passed through the fibres of the internal sphincter, and descended between this muscle and the fleshy longitudinal fibres of the rectum, until, arriving at the superior surface of the external sphincter, it passed through the fibres of this muscle, and opened through the skin, in situations more or less remote from the margin of the rectum.

The author thinks his experience warrants him to conclude,

that nearly in every case, the fistula commences in an ulcerated hæmorrhoidal tumor.

The author affirms that, in almost every case, the internal orifice may be seen, by proper management, with the naked eye. The patient must be directed to make a slight effort, as if going to stool, the surgeon at the same time separating the margin of the anus. In such a situation, a small ulcer may be almost always discovered, immediately within the orifice of the rectum, which is formed by the internal opening of the fistula.

Surgeons, when examining a fistula, with a view to ascertain the situation of its internal orifice, generally proceed under an idea that it is situated high up; they therefore direct the sound directly upwards, and push it as far as they can. It even happens frequently that the sound is forced higher up than the fistula, being pushed into the cellular substance surrounding the rectum in these parts. From the facts stated in the present memoir, it appears that the internal orifice is almost universally situated about four or five lines within the margin of the anus, and that we must not, therefore, in searching for it with a probe, or other instrument, introduce it very high, nor give it too perpendicular a direction. As the fistula runs at first between the external sphincter and the skin, before it takes a direction upwards, the probe must be carried forwards horizontally, or in a direction nearly parallel with the perinæum. Its point must be made to slide along the inner surface of the skin, and then directed towards the most inferior part of the rectum. In this manner the probe will soon be brought into the internal orifice, and may be felt by the finger, introduced into the rectum. **E.**

Observations Pratiques sur les Fumigations Sulfureuses, par Jean de Carro, Docteur en Medecine.—Vienne, 1819.—pp. 250, in 8vo.

Fumigations were already employed as early as the time of Hippocrates, as a remedy against disease; it is only of late, how-

ever, and since the publication of Dr. Galès'* work that the subject has become of general interest. This gentleman undertook in the year 1812, a series of experiments, in the hospital of St. Louis, upon the efficacy of sulphurous fumigation in the cure of psora; and the result of these is published in a thesis,† which he defended before the faculty of medicine of Paris. Although the apparatus made use of, was of course very imperfect, his success was not the less satisfactory. His experiments were repeated before professors Pinel, Dubois, Bonillon, Lagrange and others, who were chosen by the "conseil d' administration," to draw up a report. Two years subsequent to this, a second report was drafted by Messieurs Dupuytren, Leroux, Percy and Richerand, to the faculty of Paris, fully corroborative of all affirmed in the first. In both of these, a number of cases are detailed, in which these fumigations had proved completely successful.

Dr. Galès' made his first trials of this remedy in the bed of the patient, by introducing a small basin *(bassinoire)* of sulphur and nitre under the bed-clothes. The patient was stripped naked, and the head alone being permitted to remain without, the rest of the body was confined within the bed-clothes, which were wrapped very close and carefully about the neck and shoulders. Every precaution which he took, however, could not prevent the gas from finding a ready exit here; and this excited so much cough and sneezing, that he was obliged to relinquish this mode, and substitute in lieu of it, what he termed his fumigatory box *(boite fumigatoire.)* This was contrived in such a manner, as to be perfectly air-tight, when closed. On the top was a circular hole, large enough to admit the head of the patient, which was covered with a species of leather cap; and this being tied closely below the chin, prevented the escape of any gas from the box, at the same time that it permitted the patient to breathe the pure atmosphere of the room. A pipe was inserted

* Memoires and Rapports sur les fumigations sulfureuses, appliquées au traitement des affections cutanées et de plusieurs autres maladies, Paris, 1816.

† Essai sur la gale, son diagnostic, ses causes, and ses consequences medicales pratiques. Paris, 1812, 4to.

in one of the sides, communicating with the chimney, which served to carry off the products formed by the combustion of the charcoal, as well as the superfluous gases, evolved from the substances employed for fumigation. As the former of these, viz. the carbonic acid and carburetted hydrogen, tended only to irritate the skin, without in the least contributing to aid in the effect, M. Darcet improved the apparatus by separating the furnace from the interior of the box, and giving to it a distinct pipe. The bottom of the box consisted of two plates of iron, at the distance perhaps of four or five inches from each other. On the lower one of these, which was heated by the furnace, was introduced through an aperture in the side of the box, a small vessel which contained the sulphur. This was previously ignited, the aperture closed, and the vapour ascended, through a number of holes which perforated the upper sheet of iron. The temperature was regulated by a thermometer, the bulb of which was placed in the box itself, whilst the scale was without.*

A great variety of animal, vegetable and mineral substances have been successively employed for the purpose of fumigation; the most of these are now obsolete, and mercury and sulphur alone, maintain a place in modern practice. The first of these was in use as early as the 16th century, but fell again into neglect until the practice was revived about fifty years ago by a certain Labouette.† Even this has of late been superseded by the very extensive employment of sulphur; for which we are entirely indebted to the indefatigable labours of Dr. Galès. This gentleman, as we have already noticed, at first burnt the sulphur with the nitrate of potash; but finding by this mode, that a great quantity of other gases were evolved, all tending to diminish the efficacy of the sulphurous acid, he was induced to make trial of

* The hospital of St. Louis at Paris, an institution devoted solely to the cure of cutaneous diseases, contains, beside an immense Russian bath, capable of accommodating fifty patients at the same time, and a variety of others, a fumigatory box of the description above mentioned, but sufficiently large to admit a dozen persons.

† Nouvelle Methode de traiter les maladies veneriennes par les fumigations, publiée par ordre du Roi ; Paris, 1776.

the sulphur alone. Thus, in the first process, the nitric acid of the salt being decomposed, nitrous gas was given off, which uniting with the oxygen contained in the box, formed nitrous acid gas. On the other hand, the combustion of the sulphur, being favored by the presence of the oxygen of the nitric acid, there was much more sulphurous acid disengaged than in the second process. But besides these, a portion of sulphuric acid was formed by the action of the nitrous acid gas upon the sulphurous acid gas. This is evidently the same process, and affords the same results, as we witness in the fabrication of sulphuric acid in leaden chambers. When sulphur alone was made use of, the whole product consisted of sulphurous acid, with a small quantity of sulphur in the state of vapour.

We have deemed it proper to premise thus much, the better to understand the object and value of the work now before us. The author of it is the same gentleman, who so laudably exerted himself some twenty years ago for the propagation of the vaccine disease in Austria. Having perused the writings of Dr. Galès, he was induced from the astonishing cases of cure therein detailed, to adopt a similar practice, and immediately gave orders for two models of Dr. G.'s apparatus to be sent him from Paris. These were arranged in the back apartments of his dwelling for the convenience of the citizens of Vienna; to which were afterwards added several others of his own construction. The first pages of his book, contain five circulars, giving an account of the rise, progress, and success of this establishment, which he names his "fumigatoire." In the second of these, which is dated the 15th of August 1817, he says, "although I have commenced my observations since the 24th of July only, and the diseases I have treated since then, have been of the most inveterate kind, not yielding to the power of any other remedy, the results have been the most satisfactory. It is the unanimous opinion of all the physicians who have examined the apparatus, that this mode of applying the sulphur is by far the most active, and that so powerful a sudorific should be attended with the most beneficial consequences, administered in chronic diseases

of the skin, of the joints, glands, lymphatic system, and in various arthritic affections."

"All fear of contracting the disease, of those who have used the bath previously, must vanish in the eyes of the most superficial observer, as every part of the patient's body in contact with the apparatus, is covered with his own linen."

"The temperature experienced in the apparatus is from 30 to 33° of Reaumur; and this is so far from being unpleasant to the patient, or augmenting that insupportable pruritus, so common in affections of the skin, that he seeks to prolong the half hour prescribed him for the use of the bath."

In his third letter, which is written three months later, he says, "the success of his practice has exceeded his most sanguine hopes. Several cases of herpes, which had resisted all other remedies, and had existed for many years, were cured, without any medicine given internally. The dry and scaly tetter appeared to yield much more readily than the humid, or running *(humide)*. Two of the latter kind resisted, the one 38, the other 28 fumigations. Having observed, that in all the cases of cure, the amendment was very evident after the first two or three trials with the bath, and in all those in which it failed, there was not the least change for the better, during the whole treatment; I shall not, for the future, insist upon a continuance of the fumigations, unless there is some amelioration in the commencement."

"A variety of other eruptions, less violent, have disappeared in less than ten trials with the bath."

"Three cases of itch only have occurred in my practice, which were cured by eight, ten, and three fumigations."

"However efficacious these fumigations have proved in the various affections of the skin, they are not less valuable in other diseases; as in chronic rheumatism, pains of the joints, lumbago, sciatica, &c. With a little precaution, I am enabled to employ the fumigations at all seasons of the year, and even during the severest winters."

From the good effects already experienced from the bath, Dr. de Carro is fully convinced that the ill success which attended

its use in the Charité at Berlin, by Dr. Horn,* was owing to the imperfect state of the apparatus, as well as to the condition of the patients upon whom the experiments were made. In a letter of Dr. De C., to Dr. Galès, he states, that the apparatus which Dr. H. employed was constructed at Berlin, the parts so illy adapted together, that the vapour issued from all the joints, and hence the patients (who were chiefly pregnant women) were attacked with vertigo, fainting, oppression of breathing, or even spitting of blood. The temperature of the apparatus, besides, was so illy regulated that the heat became insupportable to the patients, and the skin of some of them was rendered erysipelatous. Dr. H. in passing through Vienna afterwards, visited Dr. De C.'s establishment, and affirms himself, that though he entered the room, which was perfectly closed, whilst one patient was actually issuing from the bath, and another was confined within, he could not perceive the least offensive odour.

The fourth report is dated in March 1818, he continues,— "Daily experience confirms what I have formerly said of the efficacy of fumigation in different arthritic affections, in lumbago, sciatica, and other chronic diseases of this species, in the itch, and various herpetic eruptions. I repeat, that in diseases susceptible of cure or amendment, the number of cases which has resisted the trials with the bath, is very small in comparison to the great number which have yielded to this mode of treatment.

My experience does not permit me to determine yet with precision, the particular species of syphilitic disease, to which these fumigations, either alone or combined with other remedies, are best adapted. I never employ them in recent affections. Some observations induce me to believe, that they may prove very beneficial in those diseases which arise from the abuse of mercury, and that they may be very advantageously given to prepare the system for the action of this remedy. The ephelides disappeared very readily under the use of the bath. Two instances of this kind, one of which had existed for five years, was cured with ten fumigations; a second, which was accompanied with pains in the

* Vid. Archiv fuer medicinische Erfahrung. Berlin, 1817, p. 553.

integuments of the head, and insupportable pruritus, yielded to five.

I have had to do with persons of all ages, from five to seventy. A little girl of six years, and a boy of five, of delicate constitution, having both some affection of the skin, which had resisted the ordinary remedies, were cured with the sulphurous vapour, without suffering the slightest inconvenience from its action. One of my most remarkable cures was effected in a person 70 years of age, who was afflicted with the most horrible herpes that I have ever seen, upon the arm and leg."

We pass over the various improvements Dr. de Carro has made upon the apparatus of Dr. Galès, and the instructions given for its employment, as we could scarcely render them intelligible to such of our readers, who have not seen the apparatus itself. From the scrupulous attention with which we have examined every part of it, we do not hesitate to say with Dr. de C., that " it is as perfect as it possibly can be rendered, and leaves nothing more to be desired, (qu' ils ne laissent plus rien à desirer.)

Following these reports, and occupying the greater part of the work, is a detail of 200 cases, chronologically arranged, in which these fumigations were employed by Dr. de C., with various success. It would occupy, perhaps, too much of our time to dwell upon these; suffice it to say, that the result has been upon the whole most satisfactory.

We extract the following, in corroboration of the efficacy of sulphurous fumigation, from the various reports of physicians on other parts of the continent, and which will be found appended to Dr. de Carro's work.

The faculty of medicine of Presburg, report, " that the most obstinate cases of herpes, which had resisted all other attempts of cure, were much improved, or completely relieved, when the fumigatory baths were added to the usual remedies for this disease. Some instances of syphilitic disease, yielded in a most astonishing manner to the same mode of treatment. Sixteen persons affected with the most inveterate itch, were cured in from five to eighteen trials with the bath. A boy, 13 years of age,

afflicted from his sixth month with this most disgusting malady, was radically cured."

Doctor Louis of Odessa in Russia, communicates in a letter to Doctor de Carro, the case of a person, who was affected for three months with rheumatism of the right arm and knee, to such a degree, that he was unable to walk, and lost completely the power of moving his arm, which was reduced to one-third its natural size. This man, after using the bath most perseveringly 75 times, was gradually, but completely cured. Others, who laboured more slightly under the same disease, were relieved in seven or eight trials with the bath.

Dr. Grantz of Hungary, writes to the same—" My first patient was a priest, 21 years of age, who has come here from a great distance to make trial of the bath, on account of a large scrofulous tumor in the neck, which has resisted all other remedies. This tumor was of the size of ten ordinary fists, " dix poings d'homme," the respiration was so much impeded by it, that he could not walk fifty paces without reposing. After three fumigations, it was diminished to one third its former size, his respiration was quite relieved, and in fifteen more, the tumor almost disappeared, and the patient returned to his home."

Dr. Stiller, physician to the hospital de Miseri corde, at Vienna, reports, that of 27 persons affected with the itch, 24 were cured with from ten to fifteen fumigations; the remaining three requiring but from four to six. He further remarks, " nine patients labouring under arthritic diseases, have experienced great relief from the sulphurous fumigations; six have been cured, the condition of two others much improved; one only, whose disease has been more inveterate, has experienced no amelioration. A patient, afflicted with syphilitic herpes, was cured in 28 fumigations, with the aid of calomel

Two cases of sciatica have resisted hitherto 15 trials with the bath. A complete paralysis of the lower extremities originating in arthritis has been so much relieved by the bath that the patient can now move both his limbs, and with a little support maintain himself upright.

'In none of these instances, with the exception mentioned, was there any medicine administered internally.'

We would refer our readers to the book itself, or to the "Gazette de Santé," for two most interesting cases of syphilitic herpes, detailed by Dr. Galès, in which the fumigations were employed with the greatest success.

If we have occupied more of the time of our readers, with this abstract, than it was our intention upon taking up the work, it has been with the wish of impressing upon them the value and importance of the practice therein recommended, and the hope that its benefits may soon be extended to the afflicted on this side of the Atlantic, by the speedy introduction of the apparatus into our hospitals. B.

Elements of Medical Logic, illustrated by practical proofs and examples; including a statement of the evidence respecting the contagious nature of the Yellow Fever. By Sir Gilbert Blane, Bart. F. R. S. &c. &c. London, 8vo. pp. 219. 1819.

Sir Gilbert Blane has long maintained the character of one of the most enlightened physicians, and most elegant medical authors of Great Britain. In none of the many valuable works with which he has enriched the profession of medicine, has he established his claims to this distinction in a more triumphant manner, than in the one before us. For depth of science, for force of reasoning, for vigorous originality of thought, and elegance of diction, the "Elements of Medical Logic" have few parallels in our language.

In an ingenious introduction, Sir Gilbert Blane vindicates the healing art, from the aspertions of those who condemn it as useless and inefficient. Contrary to the opinion of those who contend that the rescources of nature are all-sufficient, he shews that even brute animals have an instinctive conviction of her insufficiency, and a remarkable sagacity in the selection of medicines. He proves very satisfactorily, that the untutored savage, though subject to fewer disorders, and exposed to fewer injuries, than the

civilized man, does not usually attain the same age; and suffers from the ravages of diseases which are completely under the control of the medical art. And he concludes the refutation of this objection by shewing, that if the powers of nature were all-sufficient, all mankind, or at least a large majority of them, would die of old age.

In reply to the objection, that the healing art is inadequate to the removal of diseases, he proves very conclusively, that most disorders are more or less under the control of art; and that there are diseases which would long since have effected the extinction of the human race, had they not been resisted by the power of medicine. In addition to this, he adduces instances equally convincing from the history of preventive medicines; and shews that it is impossible to calculate the immense benefit resulting from the various means which have been devised for obviating and extinguishing infectious disorders.

He then proceeds to shew, that the abuse of the healing art is no objection to it; that the great mass of errors with which medicine has been encumbered, is no evidence of its not possessing much that is valuable and true; and that the credulity which has led to the adoption of so many crude and fallacious theories, and modes of practice, is no just ground of reproach to the science itself.

He acknowledges, however, that there are inherent in the science, many sources of uncertainty and fallacy; that there is great difficulty in ascertaining and arriving at truth in medicine; and hence he shews the necessity of careful investigation, and deduces the importance of settling the principles upon which this investigation should be conducted. In this interesting and ingenious manner he introduces his subject to our attention.

After some excellent general remarks on the principles of belief and the rules of evidence; and after shewing that in prosecuting researches into nature, there is a great difference in the degree of precision and certainty of which they are respectively susceptible; that those departments in which the subjects of enquiry are most simple and invariable, will be most easy of investigation; and that, therefore, there are many, and great obstacles

to physiological researches, because the living body is possessed of peculiar attributes of so various and fluctuating a nature as to put their influences and combinations beyond the reach of calcution; he proceeds to enumerate the properties peculiar to animated nature. These vital energies he arranges, and considers in the following order, viz: 1. The Generative; 2. The Conservative; 3. The Temperative; 4. The Assimilative; 5. The Formative; 6. The Restorative; 7. The Motive; 8. The Sensitive; 9. The Sympathetic. This arrangement differs from all those generally received, in that it is founded, not on an enumeration of functions and organs, but on the principles pervading and regulating the whole animal system. It comprehends all the principles of disease, as well as of health, since it includes all the powers of life; and, we think it presents an admirable basis for a nosological nomenclature.

The author, considering the *generative* energy as emphatically "the mystery of nature," forbears to investigate it. He considers Blumenbach's doctrine of a *nisus formativus* the most rational on the subject of generation, as it is rather an exposition of facts than a theory. He next enters into an enquiry respecting the nature of the *conservative* principle. He discards the idea of those physiologists who resolve it into nothing more than the antiseptic power of motion, and of the constant fresh supplies of food which the body receives. He shews clearly, that the causes are inadequate to account for the continual preservation of the living system; and refers this wonderful phenomenon to a principle inherent in living matter, and which John Hunter emphatically calls the *living principle.*

The *temperative* energy he defines, that by which the animal body is enabled to maintain an equable temperature. He shews this to be the most steady and invariable of the properties of life; as, whatever difference there may be in other respects between animals of different species, there is hardly any in the healthy standard of their temperature. He denies that chemical causes alone can account for the production of animal heat; and, although he does not exclude them altogether, he believes that the heat of the living body is chiefly generated by the vital

energies. He therefore considers oxygen as contributing but little to the production of heat, unless as a stimulus to the living power which generates it. He considers the excitability of John Brown's system to be, in some sort, only another name for the conservative principle; and very justly remarks, that this distinguished theorist erred, not in resting his system upon a false, but upon *too narrow* a basis. The *assimilative* process, including the changes wrought in digestion, in secretion, and in the preparation of materials for the growth and repair of the body, Sir Gilbert Blane believes to be identical with, or perhaps rather analogous to, the processes of the Voltaic battery. He therefore conceives that these changes may be effected by the agency of something like galvanism; and that each gland may be furnished with a sort of Voltaic apparatus for effecting the specific changes which the organ is designed to produce upon the fluids. This idea was first advanced by Professor Braudis of Holstein, and has been ingeniously supported by Dr. Wollaston, and Sir Everard Home. He of course, discards the doctrine maintained by Mr. Brodie, and Dr. Wilson Philip, which ascribes assimilation to nervous influence, and he enters somewhat at length into a refutation of it.

By the *formative*, he means that organizing or plastic power, by virtue of which every part of the animal body is organized, every organ moulded to its due form and adapted to the function for which it is intended. After some judicious remarks on the mysteriousness of this wonderful power, and the absurd ideas into which physiologists have been led by the vain desire of explaining it; he makes some good observations on the manner in which the growth and repair of the body are effected.

By the *restorative* principle, he designates that power which the body possesses of repairng the injuries and diseases to which it is subject. As the most remarkable evidences of the operation of this principle, he adduces sleep and the vis medicatrix naturæ; upon both of which he makes some very interesting remarks.

He continues to examine severally the different heads of his division of the energies of life, with the same ingenuity, the same philosophical acumen, and the same depth of learning, of which we have already presented our readers so many specimens. To

enter into an analysis of the remaining divisions would occupy too much of the space which we wish to devote to another, and more important part of the work. We must, therefore, refer our readers to the work itself; assuring them, that if they have any taste for true inductive research, and can admire bold originality of mind united to logical precision and rhetorical elegance, they will find in this section many valuable specimens of all these qualities.

The second section is taken up with some excellent observations on the principles of philosophising, as applied to medicine; and concludes with an enumeration of the obstacles which have obstructed and retarded its improvement. He then proceeds to consider these obstacles more in detail, and in the following order, viz: I. The errors and abuses arising out of false or misapplied theory. II. The great diversity observable in the constitution of individuals. III. The difficulty of appreciating the efforts of nature, and of discriminating them from those of art. IV. Superstition. V. The ambiguity of language. VI. The fallacy of testimony.

The fifth section is the most interesting, as it embraces the important subject of the contagious nature of Yellow Fever. Under this head, he exemplifies the evil tendency of the ambiguity of language, by the unprofitable disputes that have arisen from the abuse of the term *Yellow Fever*. He shews that this name has been given to *three different fevers*, which, however, agree in the circumstance of having a yellow skin. He restricts the term to the MALIGNANT EPIDEMIC of the West Indies, or, as it is sometimes called, the TYPHUS ICTERODES; and contends that this form of fever is decidedly *contagious*. After discussing the presumptive proofs of the foreign origin, and contagious nature of this epidemic, he takes a view of the facts which go to establish this doctrine. We cannot persuade ourselves to abridge the reasoning of Sir Gilbert Blane on this point, and beg leave to present it at full length.

"1st, It has never shewn itself in the first instance, but in a sea-port town; and never in the interior of the country, whether island or continent. 2dly, It has, in most cases, been ascertained, that it has

made its appearance in that sea-port, after the arrival of one or more ships; either under those peculiar circumstances which engender infection, or conveying the infection from ports where it had already existed Most of the great epidemics of the West Indies, North America, or Europe, can be traced to one or other of these sources. If it cannot in every instance be traced, this may happen from the want of historical facts: neither does it follow that infection does not exist, though it cannot be traced, for nothing is so subtle as infectious *effluvia*. It is well known, that small-pox and measles find their way where there is no possibility of tracing the source of the infectious matter; but will any one deny the existence of small-pox or measles in a family, because the source of them cannot be traced?

"3dly, No part of the population of the towns where it has broke out, has been affected, but such as had communication with shipping, directly or indirectly.

"The authorities for this opinion, besides the strong ones already quoted, of Dr. Mitchell and Dr. Living, are the testimonies and writings of Sir James M'Gregor, Sir James Fellowes, Mr. Pym, Sir Joseph Gilpin, Dr. Steward of Grenada, Dr. Gordon of St. Croix, Dr. Arejula of Madrid, the French commissioners already mentioned, and many other equally candid, competent, and honourable men, who had the best opportunities of closely investigating the subject.

"The doubts have been maintained more plausibly in the West Indies than in the temperate climates, on account of the resemblance of this epidemic to the endemic, and sporadic fevers of those colonies; but the testimonies of Mr. Pym, Sir Joseph Gilpin, Don George Juan, and Don Antonio D'Ulloa, and others, go directly to the point, and cannot be invalidated without impeaching the moral character of these honourable men, a species of argument, however, which it is to be deplored, has not in every instance been abstained from by the partizans of this question.

"With regard to North America, evidence the most overpowering on this subject, is to be met with in the Medical and Philosophical Register, in 4 volumes, published at New-York, in the year 1814. There are in this work such a multitude of clearly ascertained facts in proof of its foreign origin, and contagious nature, that there is not room here even for the bare recital of them. My own humble efforts have not been wanting in the same cause.

In the year 1798, I wrote a letter to Mr. Rufus King, minister from the States of America to the British court: and, in the year 1805, another to Baron Jacobs, minister from Prussia, for the information of their respective governments. In these letters, I laid particular stress on what occurred regarding a French ship taken in battle on the coast of America, in May 1795; on board of which, this fever, or its infection was found; and was communicated to the seamen of the British ship Hussar, by the men in health, who were shifted into her from the prize. It is evident that if it could be proved that this fever is communicable from one ship to another at sea, such a proof of the reality of contagion would be of the nature of an *experimentum crucis*, there being no possibility of land exhalations to account for it; such I then considered, and still consider the facts of this case to be; they were, however, so strongly and speciously contested by Dr. Bancroft, as greatly to frustrate the impressive effect which my statement was calculated to produce; the reader will be able to judge of the solidity of his objections, from an annotation at the end of this work. I feel to myself that I was so far from making too much advantage of these facts, that I might and ought to have availed myself of them still more. I might have adduced them as a very striking illustration of the incompatibility of this disease with a certain temperate degree of atmospheric heat; for the range into cool and pure air, in proceeding to Halifax, did, in a very short time, first deprive it of its malignity, and then of its infectious nature, so as entirely to extinguish it. The few that were seized after arriving at Halifax, might have imbibed the poison in the warmer latitudes through which they passed. It was on the strength of such facts as these, that in my conferences with the members of the British government, and in my correspondence with those of Russia and Prussia, I ventured to assure them, that in none of those countries was there any thing to fear from the importation of this pestilential epidemic, which in the end of the last century and the beginning of this, had so afflicted the West Indies, North America and Spain, as to excite a general alarm throughout Europe.

"There have occurred, since the period alluded to, facts equally conclusive, regarding the communication of this disease from one ship to another. It will be enough to specify one or two. A French ship of war, the Palinurus, lying at Martinique, severely

affected with the Yellow Fever, was ordered on a cruise to try the effect of sea air on the disorder. She fell in with and captured the Carnation, a merchant ship, on her passage from England, part of the crew of which, were seized with the fever while at sea. Another French ship of war, in which this fever prevailed, both at St. Domingo and on the passage to Brest, made prize of a merchant ship from the Mediterranean, off Cape Finisterre, and having without shifting the prisoners, sent a party of their own seamen to navigate her, the crew of the prize caught the fever, and almost all died of it. The men having been seized on board of their own ship, makes this a stronger case than the other, in which this circumstance is not mentioned; had they been taken ill on board of the capturing ship, it might have been said, that it was from the exhalations of the hold or stores.

"There is still another useful remark, which I did wrong in omitting in my statement of fourteen men sent from the Hussar to navigate the prize—nine died before reaching Halifax, a passage of twelve days; the other five were sent to the hospital, where some of them probably died.

"4thly. The effect of quarantine regulations, and vigilant police in shutting it out, and repelling its first assault, and the equally effectual and salutary result of shutting up in the midst of infection. All these good effects have been experienced at Gibraltar, and elsewhere. It has already been mentioned how it was arrested there, *in limine*, in the years 1810 and 1813, and that many families were preserved in the midst of the desolating epidemic of 1804, by cutting off all communication with the garrison and inhabitants; the effect of seclusion at Cadiz, has already been mentioned; the persons in the jail, hospital, and poor-houses of Philadelphia, remained exempt from the pestilential epidemic in its utmost rage, all external intercourse having been prohibited. The same was observed of prisoners of war at Jamaica. The American Register abounds with innumerable and irrefragable proofs of the good effects of seclusion and quarantine, and it ascribes to the more vigilant execution of the regulations of the latter, the exemption from it at New-York since 1805, at which time, as well as in 1801, it had insinuated itself in consequence of the loose measures of the quarantine, which, by a singular and unaccountable infatuation of the American government, had been put under the direction of professional persons, who avowed their

disbelief in importation and contagion. It may also be remarked here, that during the whole of the American war, no epidemic occurred, which is ascribed to the intercourse with the West Indies having been suspended."

Sir Gilbert Blane then proceeds to examine the objections to the doctrine of contagion, and to point out the sources of error by which the advocates of the incommunicable nature of yellow fever, have been led into that mischievous delusion. Never perhaps have these objections been so completely refuted, and the contagious character of yellow fever so triumphantly established. We consider the question as positively *settled*; and, to use the language of our author, " we venture to challenge any candid, intelligent, and unbiased man, whether in or out of the profession, to open his eyes, and deny that this disease is contagious; and if it be not, we confess that we have lost every faculty of distinguishing truth from falsehood, or discerning light from darkness."

D.

Philosophical and Chemical Researches on the use of the Prussic Acid, or Hydro-Cyanic Acid, in the treatment of diseases of the breast, and particularly in Phthisis Pulmonalis. By F. Magendie, M. D. &c. Translated from the French by James G. Percival, M. D. 12mo. pp. 86. New-Haven. Howe and Spalding, 1820.

Withering observes, that our most valuable medicines are derived from the most deadly poisons. A glance at the list of our herculean remedies is sufficient to show the correctness of this observation. Aware of the general truth of this remark, Dr. Magendie has applied himself to the investigation of the medicinal powers of some of the most poisonous articles known; and the success of his researches has been such, as to entitle him to the thanks of the profession.

Prussic acid, is now known to be the most promptly fatal poison we possess. When perfectly pure, and brought in contact with

a wound, or simply dropped on the tongue of an animal, it destroys life almost instantaneously.

With such an article, it would appear, at first sight, madness to interfere, much more so to use it as an internal remedy. When diluted, however, to a proper degree, we have now sufficient evidence, to regard it as a most valuable auxiliary in the hands of a prudent and intelligent physician.

The first 17 pages of the volume before us were published a few years ago, in England,* and republished in the first volume of the Medical Recorder, page 575. To that account of the prussic acid, Dr. Magendie has, in this little book, added a body of powerful and instructive evidence in favour of its remediate virtues. That those of our readers who may wish to prescribe this powerful article may have a view of the evidence upon which its medicinal character at present stands, we proceed to give a summary of the remaining portion of this volume.

In hooping-cough, the prussic acid has been given with decided advantage. Dr. F. Philiber Fontenelles says, that he gave the acid prepared according to the method of Scheele to four children affected with hooping-cough, and that the effects were wonderfully beneficial. He put three drops of the acid into one ounce of distilled water, and gave a spoonful of it every two hours. The children were considerably relieved in the short space of twelve hours, and "on the fourth day from commencing the use of this remedy the cough had entirely disappeared in two of them, and in two others some days later." †

In a case of pleuro-peripneumony in the clinical institution of Padua the prussic acid was prescribed by Professor Brera, with very good effects. After having drawn off 18 ounces of blood, at two bleedings "30 drops of the acid in an emulsion of gum arabic were prescribed, and taken during the day, and 12 more during the night; the day after, the urine became abundant and sedimentous, after which the expectoration diminished, the respi-

* Journal of Science and the Arts for 1818, No. VIII.

† Description of the chicken pox in Milan, 1817, by F. P. Fontenelles.

ration became more free, the cough less fatiguing, and the pain in the side ceased gradually."*

It is also asserted in the same thesis that Professor Brera "derived the greatest advantages from the use of the prussic acid in inflammation of the bronchiæ, catarrh and phthisis." A noble lady affected with incipient phthisis, "was taken with an hæmoptysis so abundant, that she was soon near expiring. Bleedings were used in vain. Dr. Brera then prescribed 100 drops of the prussic acid in the form of pills, to be taken during the night, which arrested, very promptly, the hæmorrhage." The acid was afterwards continued for 15 days in the dose of 30 to 50 drops in twenty-four hours, and the woman was entirely cured of her pulmonary affections. Dr. Brera has also treated cancer with prussic acid, and, in some cases, with advantage. Dr. Manzoni relates the case of a lady, aged 27 years, who came to Padua to be treated by Dr. Brera for a chronic affection of the uterus. She experienced very excruciating pains in the fundus uteri, and a mucous purulent matter issued from the vagina. The os tincæ presented to the touch a number of inequalities, and the feet were very hot. The menses appeared irregularly. She had suffered much from constipation and hæmorrhoidal tumors. On the second day of the disease she was taken with a very profuse uterine hæmorrhage. "None of the usual remedies in such a case were of advantage. If the hæmorrhage diminished the pains of the uterus and the hæmorrhoids became intolerable, and *vice versa*." In this situation Professor Brera gave her ten drops of prussic acid in the form of pills, every hour, and directed the patient to continue the use of them until a marked effect on the vital forces was produced. "She had taken but twenty drops when irregular palpitations, great anxiety and vertigo came on. An abundant sweat breaking out soon after, all the symptoms of her disease speedily disappeared. She now felt no pain in the uterus, the puriform discharge became serous, the neck of the uterus no longer offered too elevated a temperature, and the inequalities yielded gradually to slight injections of prussic acid."†

* De precipuis acidi pruissici et aquæ cohobatæ Pruni Lauro-cerasi medicis facultatibus observationibus comprobatio. J. A. Manzoni, 1818.

† Brera, Prospetii. Clinici.

Dr. Manzoni affirms that observations, tending to demonstrate the efficacy of prussic acid, in lessening the action of the heart, diminishing febrile heat, and combatting violent inflammations, are now very numerous in Italy. Dr. Brera has also remarked, that this acid was of great advantage in expelling lumbricoides. Dr. Magendie observes, and we think with much justice, "that the form of pills to which this learned physician (Dr. Brera) seems attached, appears to him one of the most defective for administering the prussic acid; for the great volatility of this substance does not permit it to remain a long time confined in the mass of pills, particularly when the temperature is a little elevated." This is doubtless the reason why the dose could be carried to 100 drops in twenty-four hours without any sensible injury. Dr. Granville, in a work* recently published in England, gives a considerable body of evidence demonstrative of the medicinal powers of the prussic acid. He relates several cases of advanced phthisis, in which the acid ameliorated all the symptoms to a very considerable degree. In two cases the disease appeared to have been arrested in its progress, by this remedy. "A young man and a young woman presented themselves at the gratuitous consultation of Dr. Scudamore with all the appearances of confirmed phthisis: they both experienced a fatiguing cough, emaciation, very frequent pulse, nocturnal sweats, loss of strength, purulent expectoration, and that particular form of the nails which usually accompanies these various symptoms. The Doctor gave to both the prussic acid, in the dose of 10 drops a day, and had the satisfaction of obtaining the best effects from it. These two patients perfectly recovered."

Effects of prussic acid on hectic and sympathetic cough.

Three cases are related, under this head, in which the prussic acid was used with complete success. The first case, was that of a woman who had been affected for upwards of two years with a continued and intense cough; and with chronic pain in the liver. After having for a long time suffered from this cough, hectic fever developed itself, with two daily exacerbations; flushing of the

* Further Observations on the internal use of prussic acid, London, 1819.

cheeks and night sweats. "The expectoration was irregular, sometimes thick and abundant, sometimes almost wanting, and entirely mucous; loss of appetite, loathing of animal food, and great weakness. The acid was given in doses of eight drops, and the cough was so sensibly affected, that on the night of the second day she had six hours of uninterrupted sleep, without a single moment of coughing, which had not happened for ten months." By the time that she had taken 100 drops of the acid, her appetite was completely restored; and at the end of six weeks she had lost every distressing symptom.

Mrs. Gooby, aged 40 years, had experienced a recent abortion and lost her husband at the same moment. After suffering a good deal from hæmorrhage and distress of mind, she was seized by a continual fever attended with cough, anorexia, and watchfulness; she appeared disposed to become phthisical. She took the prussic acid in the usual way; and on the second day the cough was already considerably diminished. In a very short time, she left her bed in a state of satisfactory health.

"Sarah Buck, aged 34 years, has experienced an affection of the ovary of the left side; for about ten months since she has felt a continual pain in the left hypochondrium shooting occasionally through the abdomen; she gradually wasted away, and had a continued fever; an extremely fatiguing cough began soon after, and the pulse indicated a sympathetic consumption." She began to take the acid on the 15th January, and by the 12th February she was entirely free from the cough and the symptoms attending it.

Dr. Scudamore, in a letter to Dr. Granville, relates two cases in which he used this powerful agent with the greatest success.

"A young woman aged 20, subject to a cough during winter, was attacked with typhus. The most alarming symptom was, the appearance of sub-inflammation of the brain, indicated by redness of the conjunctiva, brilliancy of the cornea, strange expression of the features, and attitudes, a violent delirium at intervals, and a hard and frequent pulse. Two bleedings, one of six and the other of five ounces, were made in twenty-four hours; leeches were applied to the temples; cold applications made to the head, and other means to reduce inflammatory action were employed.

The danger was, however, not avoided, and convalescence appeared only after one month from the first attack: a cough now began to appear, which, by degrees, became intensely severe; it returned with the paroxysm of hectic fever twice in twenty-four hours; profuse perspiration, at the end of each paroxysm. All the known means were resorted to ineffectually; and the hectic and cough increased rapidly. Dr. Scudamore prescribed the prussic acid in doses of 8 drops in twenty-four hours. All other remedies were abandoned, and the acid gradually increased to 24 drops in twenty-four hours. "By degrees the frequency of the pulse diminished, the expectoration, which was copious and puriform, was lessened, and disappeared at the end of three weeks; the hectic fever had ceased after about ten days, so that in the interval of three weeks, and by the effect of the prussic acid alone, the patient recovered her health sufficiently to go and complete her convalescence in the country."

The other case related by Dr. Scudamore, is that of a young gentleman, aged 10 years, tall and delicate, who was attacked with fever, which took at first the continued form, but at last became remittent. The symptoms of his disease were extremely variable. At first symptoms of an inflammatory condition of the brain manifested themselves strongly. The head was relieved after a week of proper treatment. The lungs then became the principal seat of the disease; the breathing was alarmingly oppressed, and the cough, which before was trifling, became continual and strong. The pulse was increased. Depletory measures were resorted to, and procured some relief. Two days afterwards the pulmonary inflammation became worse, and respiration more frequent. More blood was now drawn with manifest advantage. This relief was however of short duration, and there was developed a certain hectic febrile state of irregular type. "There was a slight chill, followed by heat on the skin and flushing of the cheeks, twice in twenty-four hours; the sweats were excessively abundant; expectoration very scanty; great general debility. The prussic acid was now given; at first in the dose of 4 drops in twenty-four hours. The body was washed twice a-day with tepid vinegar and water. Ass's milk at discretion, with cream

and a little bread, formed the regimen. The dose of the acid was gradually carried to 8 drops." On the third day of using the acid, debility, general coldness, and complete dilatation of the pupils, obliged the Doctor to suspend the use of the acid. In three or four hours these symptoms disappeared, and the use of the remedy was resumed. In fifteen days all the signs of convalescence were developed, and the breast was perfectly free.

In Asthma, Spasmodic and dry cough, and hooping-cough, the prussic acid has been employed both in England and in France, with peculiar advantage. A case is here related of an old asthmatic patient in whom the acid produced the most excellent effects.

In coughs arising from neglected colds, the prussic acid has evinced itself a valuable remedy. Several cases are mentioned in confirmation of its usefulness in such affections. Sarah Roberts, in the fifth month of pregnancy, was much harassed by a violent convulsive cough. She took the prussic acid in small doses; it produced the most happy results; in a short time she was entirely freed from her cough. Her state of pregnancy did not at all seem to influence the action of the medicine.

The author has introduced a letter from Dr. Thomson, author of the London Dispensatory, to Dr. Granville, in which he states the result of his experience, relative to the remediate powers of the prussic acid. He has prescribed the prussic acid in a great number of cases with various success; the advantage, however, which he has derived from it, is such as to induce him to regard it as a very useful medicine, and well worthy the attention of physicians; when taken into the system, it lessens the action of the circulatory system; and in a large dose, it destroys it entirely. He has never seen its eminently sedative effects preceded by any excitation, "a circumstance which distinguishes it from all other substances, belonging to the class of narcotics." He does not think, with Orfila, that this substance is absorbed and transmitted into the circulation before it produces its peculiar effects on the system. The rapidity with which its effects follow its application to the system, seems to us a conclusive demonstration of the justice of this opinion. The diseases in which Dr. Thomson has prescribed the prussic acid, are catarrhal affections, accompanied with cough, and chronic coughs. He thinks he has derived advantage from it in a case of

hæmorrhage. In the catarrhal affections which prevail epidemically in England, and which we here call *influenza*, he has used the acid with great success. "Since I have employed the prussic acid," he says, "I have had recourse to bleeding only in very few cases, although it seemed indicated by the state of the pulse."

The first and most prompt advantage of prussic acid in catarrhal affections is to restore sleep, and to render the fits of coughing less frequent. The following day, the pulse is less active and less hard, and the cough much reduced in violence. It excites the bowels, so that during its use, it is very rarely necessary to administer purgatives a second time. Dr. Thomson gives the history of four cases in which he employed the acid with advantage. The first case, was that of a gentleman aged 37 years, who had been affected with a tormenting cough for many weeks. It was extremely violent, and of frequent recurrence. Soreness in the throat, and hoarseness accompanied the cough; all the ordinary remedies had been used without success. He was put on the use of the prussic acid, in the dose of two drops in twelve drachms of water, repeated every two hours. At the end of three days all the symptoms had vanished under this treatment.

The second case, was that of a lady, aged 40, who about two years before, was attacked "with that peculiar affection of the mucous membrane of the trachea, very well designated in its most advanced degree by the name of tracheal phthisis." Her disease was attended by a troublesome cough, a sense of dryness in the throat, threats of momentary suffocation, and general inflammation of the fauces without swelling. "These symptoms, which were accompanied with fever and great irritability, never ceased entirely." Every slight cold she took, all the symptoms were greatly aggravated. She had used a variety of remedies and modes of treatment. On visiting his patient, the Doctor found her in the midst of one of her attacks. "The coughing was more severe than usual, very frequent, and resembled that of croup; suffocation was imminent; the heat of the skin ordinary, the pulse small, quick, and intermitting, the fauces extensively inflamed, without swelling. The prussic acid was given in this formula. ℞ Prussic acid gtt. xii; rose water ℥ss; syrup of poppies ʒiij. mix, and take a large spoonful every second hour.

This prescription was continued for four days only, by which time her condition was much ameliorated, and in one month from the time of first taking it, she was perfectly well.

The two remaining cases related by Dr. Thomson, were patients affected with chronic cough, and general symptoms of pulmonary irritation. They were speedily relieved by means of this remedy.

Our author next introduces some observations on the use of the prussic acid, by Dr. M. De Kerkaradec, of Paris. Six cases are related as having occurred in the practice of this physician, strongly indicating the medicinal virtues of the prussic acid in nervous coughs, phthisis pulmonalis, hooping-cough, and generally in affections where the respiratory organs appear principally to suffer.

In America the prussic acid has not, as yet, been much prescribed. In this city, we have heard of its having been given in several cases, but the general impression here is, we believe, not much in its favour. We ourselves have used it in four cases of phthisis. In two of these the cough, night sweats, and burning heat in the palms of the hands and soles of the feet, were considerably relieved by the acid. In one case, we thought it did harm; and in another it produced no obvious effect on the state of the disorder.

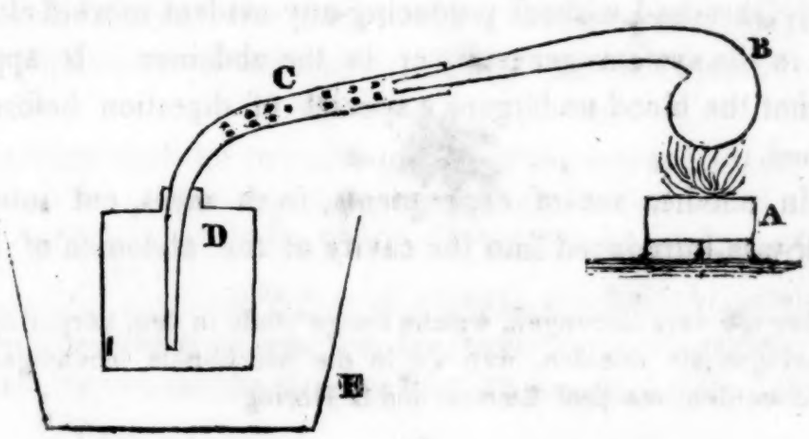
Dr. Macneven, in a letter to Professor Cooper, says "I have used the prussic acid with great advantage. It almost always removes the cough which accelerates and favours tubercular inflammation in predisposed habits, and thus in many cases prevents the disease."

Dr. Davis of Columbia, South Carolina, we are informed by Professor Cooper, has employed this remedy, in nervous coughs and phthisis with the most decided advantage.

From the experiments which have been made with the prussic acid on living animals, it appears to act by diminishing, directly, the irritability and sensibility of the animal system. In all such disorders, therefore, as depend upon a morbid increase of these vital powers, this remedy would seem to be particularly indicated. In phthisis, there is manifestly a great increase of the irritability. It was with a view to reduce this morbid irritability of the

system in phthisis pulmonalis, and hectic, that the inhalation of carbonic acid gas was prescribed by Beddoes, Thornton, and others. It is upon the same principle, that the prussic acid operates so beneficially in these affections; and we cannot but believe, that when this substance shall be better understood, in all its management, it will prove a very powerful and valuable remedy, in all such diseases as are connected with a morbid irritability of the system.

Dr. Magendie prefers the pure prussic acid prepared by the process of Gay Lussac, and diluted with six times its volume of distilled water, or eight times its weight of the same, to that prepared by the process of Scheele, which is not sufficiently uniform in its power, on account of the liberty which the process leaves to the preparer. The difficulty of procuring this acid, of an uniform strength, and the still greater difficulty of preserving it from deterioration, has hitherto been a very great objection to its employment in practice. It is therefore very desirable, that so valuable and powerful a remedy as this is, should be obtained in a more permanent form than the aqueous solution, which decomposes by light, by air, and by the water itself. Professor Cooper has made a preparation that promises to answer the purpose. The following is his process. Into strong rectified alcohol, surrounded by ice, throw a quantity of the gas obtained from prussiat of mercury. One ounce of such alcohol will at 32° of Fahrenheit, absorb all the gas that can be procured from 400 grains of prussiat of mercury slightly moistened with about 50 or 60 drops of muriatic acid, and exposed to a gentle lamp heat, in an apparatus such as this,



A a lamp. B retort. C chalk and dry muriate of lime. D vessel containing alcohol. E vessel containing ice, surrounding the alcohol.

Not more than two drops can be given in a day, in doses of one-sixteenth part of a drop.

E.

Deutsches Archiv für die Physiologie—Vierter band, vierter heft herausgegeben. Von J. F. Meckel. Halle und Berlin, 1819.

German Archive for Physiology. By J. F. Meckel. vol. iv. No. 4. pp. 148.

This is one of the most celebrated journals published in Germany. It is devoted exclusively to physiology, and supported by the contributions of nearly all the eminent physicians of that country.

The first article in the number before us is "*An enquiry into the changes, which certain substances produce upon the body, and undergo themselves, when introduced into the cavity of the abdomen of living animals.* By Professor Emmert and D. Hoering."*

1. In the first set of experiments, fresh cold blood was introduced into the cavity of the abdomen of different animals. At different periods after the introduction of the blood, the animals were killed, and the state of the abdomen examined. From these experiments, it appears, that a considerable quantity of blood, introduced into the cavity of the abdomen, is, in general, speedily absorbed without producing any evident morbid changes either in the system generally or in the abdomen. It appears, also, that the blood undergoes a species of digestion before it is absorbed.

2. In another set of experiments, fresh meat, cut into fine pieces, was introduced into the cavity of the abdomen of living

* Ueber die veränderungen, welche einige stoffe in dem körper so wohl hervorbringen alo erleiden, wen sie in die bauchhöhle lebendiger thire gabracht werden, von prof. Emmert und D. Höring.

animals. From these experiments it appears, 1. That flesh, brought into the abdominal cavity, produces a considerable degree of irritation in the peritoneum. 2. That the flesh suffers a diminution in the weight, corresponding in some degree to that which it suffers in the stomach by digestion. The author observes, that the digestion which the flesh underwent in his experiments, was far less than that observed in Smith's experiments.*

3. Urine was introduced into the abdomen of several animals. These experiments shew, that urine is absorbed from the cavity of the peritoneum, and that small quantities of it may be brought into this cavity without producing death in all instances.

Drs. Autenreith and *Sury*† long ago instituted a set of experiments, upon the effects of bile, effused into the cavity of the abdomen. They conclude that it is always a fatal accident. *Fryer*,‡ in some observations he has published, renders it probable that this is not the case; that death does not necessarily follow an effusion of bile into the cavity of the abdomen. Our author relates a number of interesting experiments on this point. Bile of various appearance and consistence was thrown into the abdomen of living animals.

From these experiments it appears: 1. That bile poured out into the abdomen is speedily absorbed, and carried into the circulation. 2. That the following effects are produced, by its action upon the peritoneum: Pain, resembling those of colic, attended with great prostration of strength, especially in the hinder extremities; great sickness, followed sometimes by vomiting; the abdomen becomes bloated; the pulse small and contracted, and sometimes, though rarely, full and frequent; flushes of heat are occasionally noticed; but there never is observed the least bluntness or diminution of sensibility; death, or a very slow recovery follows.

In animals that die in consequence of the introduction of bile

* Medical commentaries of a society of physicians at Edinburgh, 2d decade, vol. 10, page 241.

† Sury praes: Autenrieth diss: inauguralis medico-chirurgica de sanandis forsan vesiculæ fellæ vulneribus, Tubengæ, 1803.

‡ Salzburger med. chirurg. Zeitung, 1816, Th. 1. s. 55.

into the abdomen, there is but little or none of this substance found in this cavity. Nor is there any obvious morbid change found in any particular part of the body. 3. Finally; that the bile produces the above mentioned injurious effects, only when it comes in contact with the peritoneum, and not when brought in contact with the stomach and the flesh of external wounds.

Various other experiments of the same character are related by our author. The following is a recapitulation of the deductions which the author draws:

1. Substances brought in contact with the inner surface of the peritoneum, whether belonging to the body or foreign to it, are absorbed, even in considerable quantity and with rapidity; stimulating articles are much more rapidly absorbed than mild ones.

2. Many articles when brought into the cavity of the abdomen, penetrate, or percolate through the peritoneum, and come in immediate contact with the parts lying beneath this membrane.

3. Substances introduced into the abdomen, suffer occasionally remarkable changes in their composition; calomel is in part converted into a black oxyde of mercury; oil acquires an appearance as if it had stood a long time exposed to the air; blood is converted into a blackish watery fluid; meat becomes pulpy and changed into a yellowish semi-fluid matter, of an acid quality, resembling pus in its appearances.

4. Bile, and urine, although always occasioning much disturbance and distress in the system as soon as introduced into the abdominal cavity, do not necessarily prove fatal in their effects.

5. Although the peritoneum be destitute of nerves, and totally insensible to pain from wounds, yet horse-radish, muriate of iron, &c. when brought in contact with its inner surface occasion excruciating pains, which, like the pain which arises in organs that are supplied with nerves from the ganglionic system, is attended with nausea, and not unfrequently with vomiting. These pains are probably the effect of the operation of these substances upon the nerves which lie behind the peritoneum.

6. The peritoneum enters readily into an inflamed condition, but this is seldom manifested by redness and an evolution of new

blood-vessels. The inflammation is more especially manifested, by the changes which the secretions of the peritoneum undergo, and particularly by the exudation of a lymph which consolidates very rapidly, evolves blood-vessels, and acquires acid qualities. This corresponds with the observations that have been made, respecting the character of the fluids found in the abdomen of those who have died of peritoneal inflammation. This lymph seems to produce the changes which flesh, introduced into the abdominal cavity undergoes; and it acts upon it, apparently, like the gastric juice in the stomach upon digestible substances. In the same manner, it would seem, the lymph which is separated in the process of nutrition,* dissolves the effete and useless parts of the organization, to render them fit for absorption and elimination.

7. From this, then, it appears that the peritoneum, and probably all serous membranes, possess a certain degree of digestive power, which is perhaps increased by inflammation; and also a high degree of absorbing power.

In our next number we shall give an analysis of the remaining papers contained in the interesting journal before us. For the present we have neither time nor room to extend the article.

E.

Correspondence between Dr. Wilson Philip and B. C. Brodie, Esq.

We have perused with interest the correspondence which has lately been published between Dr. Wilson Philip, and B. C. Brodie, Esq.†

We have been particularly pleased with the open, mild, and temperate spirit evinced by Dr. Philip on this occasion, and the firm and manly style in which his letters are written. Notwithstanding, as we think, he had just cause of complaint, we find

* *Autenrieth.*—Handbuch der empirishen physiologie, 2 Th. §781.

† Vide Johnson's Med. Chirurg. Journal, for April 1820, and London Med. and Phys. Journal, for April 1820.

no asperity or personality in his remarks, rightly judging that, although a sneer or a sarcasm may for a moment wound the feelings of an opponent, yet they can do nothing towards settling any question in science, permanently depressing a rival, or elevating one's own character. We would recommend him as a fit model to such of our professional brethren as are so unfortunate as to get entangled in controversy. We wish we could say as much of the share which B. C. Brodie, Esq. maintains in this correspondence. We wish we could have discovered stronger indications of those elevated and generous dispositions which, in literary controversies, where the only legitimate object is the development of truth, should prompt men frankly and with pleasure to acknowledge any injustice of which they may accidentally have been guilty; we should be gratified to see these feelings a little more generally diffused, in our department of the republic of letters. We confess that his letters have appeared to us to betray a petulance and abruptness which the circumstances, as far as we can judge, by no means warranted. As the question is one of considerable physiological interest, we regret that our limits will not allow of our inserting the whole correspondence. It will be our endeavour to give a brief, but faithful, summary of it.

Doctor Philip has read four papers before the Royal Society of London, three of which have been published in the Transactions of that society. After the paper, which is the subject of the present discussion, had remained several weeks in the hands of the president, during which Dr. Philip had been credibly informed that he, (the president,) both from his own opinion, and the report of others best qualified to judge, had expressed himself very favourably respecting it, he learned that the paper was not to be published by the society. Shortly afterwards he received a letter from a friend, who was in habits of intimacy with many of the members of the royal society, informing him—"I have good reason to believe that it has been reported in and out of the Royal Society, that your observations and conclusions are not correct, and that the stomach cannot be made to act by the process you have described."

Astonished at this information, as he had preserved no perfect

copy of the paper, Dr. P. applied to one of the Vice Presidents, requesting either the original or a copy. He accordingly received a copy, together with the following observations of the clerk, with the subjoined account of an experiment.

"N. B. The following is a copy of a paper written in a different hand, and pinned in page 13th over part of the account of experiment 3d, without any reference to the text whatever."

"Two rabbits, which had fasted for seventeen hours, were allowed to eat parsley. The nerves of the were then divided in the neck of each. One of them was allowed to remain quiet. A slip of tinfoil was connected to the lower divided ends of the other rabbit, and another piece of tinfoil, an inch square, was applied to the abdominal muscles, over the stomach and under the integuments, by means of a wound in the latter. The tinfoil over the stomach was connected with a wire communicating with one end of a Voltaic battery, sufficiently strong to excite slight contractions of the muscles of the fore legs. This process was continued for 5 hours, at the end of which period both rabbits were killed.

"On examining the stomach of the animal, which had been subjected to the influence of the battery, it was found much distended with food, the parsley was principally in the cardiac portion, and near the œsophagus it appeared to have undergone no alteration, and below this it was mixed with other food in the stomach, so that no accurate observation could be made on it.

"The stomach of the other rabbit was examined by the side of the first, so that they might be compared together, and the appearances were precisely the same with those which have been just described. The contraction in the centre of the stomach was somewhat greater in the galvanized stomach than in the other."

In order that our readers may form a just opinion on the subject, we think it proper here to insert the experiment 3d, in the inquiry of which the above is stated to be an exact imitation.

"Experiment made by Doctor Philip.—The hair was shaved off the skin over the stomach of a young rabbit, and a shilling bound upon it. The eighth pair of nerves were then divided, and

about a quarter of an inch of the lower part of each coated with tinfoil. The tinfoil and the shilling were connected with the opposite ends of a galvanic trough, containing forty-seven four inch plates of zinc and copper, the intervals being filled with muriatic acid and water, in the proportion of one of acid to seven of water. The galvanic influence produced strong contraction of the muscles, particularly of the fore limbs, and frequently the pain it occasioned was such that the animal cried out violently, and made it necessary for a little to discontinue the process.

"For five hours the animal continued quite free from the symptoms which follow the division of the eighth pair of nerves in rabbits. It had neither vomited nor been distressed by dyspnœa. It had not eaten any thing after the nerves were divided.

"At this time the power of the trough became much weaker, so that it produced no visible effect on the muscles. The respiration now began to be disordered. In a quarter of an hour it became so difficult, that the animal appeared to be dying. It was gasping. Acid was put into the trough till the the galvanic power became as great as at first. Soon after this the animal ceased to gasp, and breathed with much greater freedom. The galvanic process was several times discontinued and renewed, so that we repeatedly saw the gasping and extreme dyspnœa return on discontinuing, and disappear on renewing it.

"The animal seemed now much exhausted, and could scarcely raise itself. It had been held down on its side during the whole experiment. It died in six hours after the division of the nerves.

"On opening it, we found the œsophagus perfectly natural, and no food in it. The stomach was not larger than usual. The food had undergone considerable change. The appearance and smell of the parsley were gone. The smell was that of the rabbit's stomach, while digestion is going on, which is peculiar. Mr. Hastings, who has been much accustomed to examine the stomach of rabbits under various circumstances, said that digestion was nearly as perfect as it would have been in the same time in a healthy rabbit.

"The membrane of the trachea was of its natural colour, and

there was no fluid in it. The ramifications of the bronchiæ in the left lung, were quite free from frothy mucus. There was some fluid in the right lung, though it did not appear much gorged; there was one dark spot on it. The lungs collapsed imperfectly on opening the chest.

"This rabbit had not eaten any thing for twelve hours, until within three hours of the operation, during which it was allowed to eat as much parsley as it chose."

The above account of an experiment, which had been thus carefully attached to a paper laid up in the archives of the society, was anonymous. This explained the reason why the president and others had changed their opinion respecting the merits of the paper.

We believe, that there are few men who would not have felt hurt at this implied charge of gross carelessness or an intention to deceive others. Dr. P., however, behaved with great forbearance on the occasion. In an appendix to his "Inquiry into the laws of the vital functions," published more than a year after this, he mentioned an account of the experiment, which he had received, without saying from what quarter, pointing out that it did not bear on the subject in question. A year elapsed between the publication of the first and second editions of the Inquiry, when, finding no explanation given of the affair, he published in the appendix, a full exposition of all the circumstances relating to it. This took place some time in the year 1818, and it remained unnoticed until April 1819, when a reply appeared in the Quarterly Journal of the Royal Institution.

This article is entitled "A few facts relative to Dr. Wilson Philip's attack on the president and council of the Royal Society." It seems to have been written by the Editor, Mr. Brande, and has the following paragraph:

"When the paper was read before the Society, there were many members who thought it was right that one of the experiments should be repeated. Three members of the society undertook this task, one conducting the galvanic part; another, the anatomical part; and the third, who was not made acquainted which was the galvanized rabbit, was called in after the experi-

ment was over, to decide upon the stomach in which the food was most acted upon: that the experiment might be repeated with the greater accuracy, the paper was put into these gentlemen's hands, who *implicitly* followed the *directions* contained in it. The gentlemen were quite competent to the task, and each confined himself to his own department; the third, who was employed to examine the contents of the two stomachs, after an accurate inspection, was unable to detect the slightest difference between them. This result was stated to the President, to whom it was also explained, that the rabbit, which is a species of ruminant, does not digest its food, till it has gone through a previous process of maceration, and is therefore not so well fitted for such experiments, as animals that live on animal food. The manuscript dissertation of Dr. Philip was then returned to the clerk of the society; and a minute, made during the time of the experiments, was accidentally left in it."

The experiment made by Dr. Philip, and that of the three members of the Royal Society, are then fully stated and briefly commented on. At the close of the paper an experiment on a young cat is detailed, but in this experiment the termination of the nerves of the eighth pair on the cardia of the stomach were divided, and not the par vagum in the neck. An experiment which, as we are informed by Dr. Philip, "had been related to him by Sir Everard Home in a letter, as having been performed by M. Magendie, more than a year before the first edition of his Inquiry was published; and mentioned in p. 168; nor can he imagine why a different result should have been expected." The par vagum evidently conveys to the great chain of ganglions the influence of the brain. When it is divided at a short distance from its origin, the influence it conveys is cut off; but I cannot perceive how this can at all be done by dividing the particular branch of this nerve which goes to the cardiac portion of the stomach. Before it arrives at this place, it has formed innumerable connexions with the great sympathetic and ganglions. They have received from it the influence of the brain; and if nerves, going to any particular organ be divided, there are, every where, such are the precautions of Nature, numerous anastomosing branches still capable of conveying the necessary influence, as long as it

is duly supplied from its sources. We conceive, therefore, that this has not an essential bearing on the question, and cannot be fairly brought into the description. In May of the same year, there appeared in the Quarterly Journal a pretty full reply to the above statement, in a letter from Dr. Philip, addressed to W. T. Brande, Esquire. The controversy rested here until the appearance of Dr. Cook's work on Nervous Diseases in the beginning of 1820, when, in consequence of some observations in the introduction to this work, a correspondence took place between Dr. Philip and Mr. Brodie. As the reader has now before him a history of the progress of the controversy, thus far, with a minute account of Dr. P.'s experiment, and that of the three fellows of the R. S. which was supposed, entirely to invalidate all the experiments adduced by Dr. Philip to shew, "that, although when the eighth pair of nerves are divided in the neck of the rabbit, if the animal be left alone, the process of digestion immediately ceases, yet if the animal be placed to a certain degree under the influence of the galvanic agent the function is again resumed." The experiment made by the three gentlemen, was afterwards repeated by Mr. Brodie alone, with this difference, that the animal was kept under the influence of the galvanic power in a continued stream for seven, instead of five hours, with the same results. We shall now endeavour to state briefly the objections urged by Dr. P. against these experiments, and the reasons given by him, why the inferences drawn from them are not legitimate, together with the arguments brought forward by him to sustain the doctrines advanced in this "Inquiry."

After having vindicated himself from the charge of making an attack on the President of the Royal Society; he remarks on the assertion of this gentleman, "that the rabbit is a species of ruminant, which does not digest its food until it has gone through a previous process of maceration, and is therefore not so well fitted for such experiments as animals that live on animal food;" "I have, with the assistance of Dr. Hastings and Mr. Sheppard, physician and surgeon to the Worcester Infirmary; examined, by killing the animal at various periods of digestion, the stomach of about a hundred and thirty rabbits, and that as far as we could judge from this extensive set of experiments, the results of which

are stated at length, in my Inquiry, the information thus given to the President of the Royal Society, is incorrect. The rabbit is, in no sense of the word, a species of ruminant."

It is universally admitted that in such experiments, even the slightest deviation may influence the result. Dr. P. denies that the gentlemen "implicitly followed the directions contained in the Inquiry," as asserted by them.

They varied—1*st*. "In introducing the tinfoil under the skin, which was a useless piece of cruelty; the skin, when shaved, being a sufficiently good conductor of galvanism."

2*nd*. In his experiment, a continued stream of galvanism was maintained, while, in that of the three gentlemen alluded to, only occasional contacts were made between a wire connected with the other end of the battery, and the tinfoil in the neck, three or four times in a minute. But it is stated in the Quarterly Journal, that "Doctor Philip says, in his experiment, there were muscular contractions produced by the galvanic influence, which proves that he employed it, not in a continued stream, but by occasional contacts, as in the experiment made by the members of the Royal Society." Dr. P. observes, "the gentlemen who make this statement, must indeed be unacquainted with the effects of galvanism on the living animal body, when they suppose that a continued stream of galvanism, of a certain power, applied as in my experiments, does not occasion a constant repetition of contractions in the neighbouring muscles. Not only the continued stream was wanting in the first experiment, but the requisite power of galvanism was wanting in both, because it requires a much greater power to occasion repeated contractions of the muscles by a continued stream, than by occasional contacts of the metals." He likewise infers, from the fact stated by Mr. Brodie, "that there was little or no inflammation of the stomach," that it is certain that the galvanic power was either too weak, or not sufficiently directed to the stomach, "because it is impossible to see a stomach, which has for a sufficient length of time, been exposed to such a power of galvanism, as he had found necessary to produce digestion, without being struck with its inflamed appearance." It is pertinently remarked by Dr. P.—"It is a circumstance worthy of notice, that the proofs on one side, are all of a positive, yours

necessarily, of a negative kind. We only speak of what we could not have seen, had it not existed; you, of what you were not able to see, which many things, besides what you suppose, may have prevented your seeing." Dr. Philip thus concludes one of his letters:

"In case the experiment should again be repeated, I think it necessary to call your attention to the following circumstances, which are not less essential than the degree of the galvanism, and its mode of application. Although your experiments stand in opposition only to my shorter experiment on rabbits, (for as to that in which the galvanism was continued for sixteen hours, no attempt has been made to repeat it,) yet you adopt the rule which, according to my method, necessarily produces an experiment of long continuance; for your object is to employ no stronger power of galvanism than is requisite to produce a twitching in the fore legs. In my shorter experiment, which lasted six hours, the power of the galvanism was much beyond this degree, producing general contractions of the muscles, (p. 224, second edition,) and proving fatal to the animal in the above short space of time. I found it more conclusive to employ a comparatively weak power of galvanism, that the animal be longer exposed to it; but you employed this weak power, and yet, by killing the animal at the end of five or seven hours, defeated my object in having recourse to it. You wholly deviated from my principle, when you killed the animal by any other means than the galvanism. You will perceive, from all that I have had occasion to say, how far from correct, is the observation in your first letter, that you could discover but one point of difference between your experiment and mine.

"It is also proper to remark, that the old galvanic trough, which I observe in my treatise, I always used, is preferable in such experiments, to the improved pile, because, in the former, the number of plates is greater in proportion to the surface; and I have found that it is on the intensity, not on the quantity of the electric power, that the above effects on the animal body depend. See some observations on this subject, which I transmitted to Dr. Thomson, and which appeared in the eleventh volume of the Annals of Philosophy, page 117."

We think it but justice to Dr. Philip, to subjoin the following declarations:

"I hereby declare, that having often divided the eighth pair of nerves in the neck of the rabbit, and always found that it immediately put a stop to digestion, the food eaten previously to the division of the nerves being found in the stomach unchanged, however long the animal lived after the division of the nerves: I was requested by Dr. Philip to make the following experiment. Having allowed a full-grown rabbit to eat as much parsley as it chose, after a long fast, having shaved the hair off the stomach, I divided the eighth pair of nerves in its neck, and coated the lower part of the nerves with tinfoil, and bound a shilling over the stomach. It was then exposed to the galvanic influence, by connecting the tinfoil and shilling with the opposite ends of a galvanic trough of such power as to keep up a constant twitching in the fore legs. The difficulty of breathing, which always succeeds the division of the eighth pair of nerves, had come on before the galvanic apparatus was arranged. Upon the application of the galvanism, the breathing soon became free. When the power of the trough failed, or we intentionally discontinued the application of the galvanism, which we did repeatedly, the breathing always became oppressed; but was again rendered free on the re-application of the galvanism. This continued to be the case for twelve hours, after which the breathing could not be wholly relieved by the galvanism (inflammation of the lungs having supervened); and during the last hour of the animal's life, the dyspnœa was very great. No attempts to vomit occurred in this animal, except once, when the galvanic influence had become very weak, ten or twelve hours after the division of the nerves; whereas, in every instance, in which I have divided these nerves without the application of galvanism, and I have done so more than a dozen times in the rabbit, efforts to vomit always soon ensued. I am ready to attest the truth of every part of the preceding statement in any way in which I may be called upon to do so.

(Signed) CHARLES HASTINGS."

"Worcester, February 26, 1820."

"Three other medical men were present at this experiment, and particularly examined both the stomach and lungs, and expressed themselves perfectly satisfied with the result.

(Signed) C. H."

"Having, at the request of Dr. W. Philip, divided the eighth pair of nerves in the neck of two small dogs, which were allowed to eat as much lean raw mutton, cut into small pieces, as they chose, immediately before the experiment, after having fasted for many hours, I subjected one of them to the galvanic influence, by coating the lower parts of the divided nerves with tinfoil, and connecting it with one end of the galvanic trough; while the other end of the trough was connected with the region of the stomach, which before the experiment, had been shaved, and a three shilling piece bound upon it. The power of the galvanism was such as to occasion a twitching of the fore limbs during the whole experiment. The dog which was not galvanised, was immediately seized with dyspnœa, and efforts to vomit. In the other, neither was observed in the slightest degree, at any period of the experiment, except when for a few seconds the galvanic influence was intentionally discontinued, during which the breathing became very laborious, again becoming free as soon as the galvanism was restored. This dog lived above two hours. The other dog was still alive, though extremely weak, at the end of four hours, at which time it was killed by a blow on the head.

"On examining the stomach of the galvanised dog, the mutton was found in a soft, half-dissolved state, all character of muscular fibre having disappeared. The lungs, on examination, were found perfectly healthy, but rather of a florid colour. In the stomach of the other dog the bits of mutton still retained their firmness, and on being cut into, displayed both the red colour and fibrous appearance of the muscle, which did not seem to be at all diminished. The lungs were found greatly congested, and collapsed very imperfectly, the surface being covered with patches of a dark red colour.

"The above experiment was made in presence of the house surgeon and pupils of the infirmary, who all examined the state of the stomach and lungs, and expressed themselves satisfied with the result. The accuracy of the above statement I am ready to attest in any way in which I may be required to do it.

(Signed) JAMES P. SHEPPARD."

"Worcester, Feb. 7th, 1820."

REVIEWS.

CRITICAL DEPARTMENT.

Quidquid venerit obvium, loquamur
Morosa sine cogitatione.

MARTIAL.

An Essay on the Nature and Treatment of that State of Disorder generally called Dropsy. By John G. Whilldin, M. D. Philadelphia. Printed for the Author, by William Fry, 1820.

It is not our intention that publications of the character of the one, whose title we have given above, should come under the censorship of our criticisms. Our object in bringing before our readers, Dr. Whilldin's Thesis, is twofold. 1st, We consider ourselves bound to animadvert upon certain strange and unheard-of measures pursued in relation to it, by certain of the professors; and 2dly, to review a system of preparation of students for examination, which is rapidly gaining ground, in the school of Physick of the University of Pennsylvania, and which, if not checked by the interference of the regents of the establishment, will, in a very short time, lower the character of the institution, and destroy the reputation of her diplomas.

Dr. Whilldin's thesis is written with great neatness and modesty, it contains a considerable number of valuable observations, and is highly creditable to the talents of a young gentleman just entering on the practice of his profession. It was not written with the view of being published, and has only been brought before the public, for the purpose of giving a true explanation, of the causes

of difficulty experienced by the author, in obtaining his degree. The doctor, however, tells his own story so simply, and clearly, that we make no apology for transcribing his preface:

"As I would not venture the following essay before the impartial tribunal of public opinion on its own intrinsic merit, justice to myself requires a brief explanation of the circumstances that have led to its publication; and as I profess to give a plain and candid narrative of facts, I shall be pleased to acknowledge any thing that may be hereafter found to be partial or essentially imperfect in my statement.

"Having strictly complied with every prerequisite, I presented the following essay to the Dean of the Medical Faculty in the University of Pennsylvania, about the 20th of February last, and requested him to enter my name on the list of candidates for the degree of doctor of medicine, and on the 20th of the next succeeding month, having been previously notified by him, I appeared before the Faculty, to sustain the customary trial.

"During my examination, the Professor of the Practice of Physic stated to the rest of the Faculty, that he had read my thesis, and found my pathological and practical views correct, but that I had gone from the point to give a loud denunciation against Nosology; that I declared it to be abandoned by every intelligent practitioner, and attributed our difficulty in EVERY instance to its influence; that I appeared to refer to what was going on in their school, and that he objected to the personality of my observations.

"In answer to these remarks, I said, that as I was discussing opinions and not characters, I deemed it my right to speak freely; that in denouncing Nosology, I had written without the slightest reference to individuals, and that so far from adverting to the state of affairs in their school, whatever might be my opinion, I would not impertinently obtrude it on the Faculty at that time.

"With this explanation, the Professor declared his most perfect satisfaction, and concluded my examination, when I retired according to custom, and after a few seconds, was introduced by the Dean, UNCONDITIONALLY received by every member of the Faculty with the usual congratulations of admission, and requested to enrol my name in the list of graduates.

"Supposing the decision of the Faculty to be final, and believing nothing more would be required of me than to appear publicly

to receive the honour that had been awarded me, I did not hesitate to leave the city on the following day, but was greatly surprised on the evening of the 22d, at receiving from the Dean a note,* stating that he wished to see me with respect to my thesis, which, by a resolve of the Medical Faculty, would require some alterations. I returned home by the earliest opportunity, and on the evening of the 23d called on the Dean, who repeated what he had said in his note. On my declaring my perfect willingness to make any further explanation that might be considered necessary, and again disavowing any intention of personality, he declared that he and all the Faculty were perfectly convinced my remarks were not personal, but not approving of the opinions advanced in the parts of my thesis referred to during my examination, they did not wish to appear to sanction them by allowing them to remain therein.

" Supposing that freedom of inquiry was at least permitted, if not recommended, I endeavoured to defend the opinions I had advanced, but after an hour's conversation, was again told certain alterations must be made in my thesis, and requested to meet the Dean at the University on the ensuing morning, that he might show me the objectionable parts. With this request I complied, when I was commanded (with the threat that if I refused I should forfeit my degree) to remove the following portion of my thesis:

' Though there are unquestionably great difficulties essentially ' connected with the state of disease of which we are treating, I ' doubt not, indeed I feel confident that the want of success in at- ' tempts at its removal, is, in very many instances, attributable to ' the absurd practice of addressing our remedies to one particular ' symptom, without the slightest reference to the immense variety ' of circumstances under which it may occur. This, the legitimate ' offspring of Nosology, is an evil greatly to be deplored, not only ' for the mischief it has done in the practice of regular physicians, ' but for the dreadful ravages it will yet commit, in the broad foun- ' dation it has laid of the grossest empiricism.

' Among the many evidences of its absurdity, Dr. Sydenham ' has recorded one instance in his Treatise of the Dropsy. Having ' succeeded in effecting a cure in one instance (the first which he ' had to treat) by a particular plan of treatment, he says he thought

* This note was dated 20th, and bore the post mark of 21st.

'he had become master of an infallible remedy, and boldly and confidently administered it in the next case that came under his care, 'but having persevered in its use for some time, his patient gradually grew worse, became dissatisfied, and dismissed him. He 'further adds, "if my memory does not fail me, she recovered by 'the aid of another physician, who administered more powerful 'remedies." And such will be the mortifying fate of every practitioner, who addresses his remedies to a name given to a variable 'combination of symptoms, instead of watching attentively their 'many changes, and varying his treatment accordingly.

'The late Dr. Rush very well compared a physician who pursued such a course, to the Bermudian sailor, who, when he set 'out on a voyage, threw out shingles from each side of his ship, 'to serve him as guides on his return.

'When the monster Nosology reigned unopposed, it was to 'be expected that such practice should prevail; but it is truly 'astonishing, that it should still have existence, where the first 'medical precept we are taught, is "to attend to the state of the 'system, and prescribe accordingly."

'If the force of error is so strong as to prevent us from acknowledging and embracing truth, when it is placed before us in 'its strongest light, how transcendently great must be the powers 'of that genius, which can bring it forth from the depth of obscurity, in which it is sometimes buried; and, divesting himself of 'the prejudices of early education, will firmly stand as its advocate, 'regardless of the weight of opposition he must meet.

'I cannot here withhold my humble tribute of gratitude and 'praise to that great benefactor of mankind, whose gigantic powers 'shattered the fetters of Nosological tyranny, and who, while he 'warmly advocated the national liberty of his country, achieved 'her INDEPENDENCE in Medicine. Impelled by an ardent love for 'truth, and encouraged by the hope that it would finally prevail 'against the strongest opposition, Dr. Rush not only discovered 'and embraced it, but stood almost alone in its defence; and so 'far from being discouraged by the opprobrious epithets that were 'so abundantly showered on him, viewed them as incentive to increased efforts, and became stronger by resistance. But though 'he has done much, though he has routed the enemy, and made 'his final extermination inevitable, the conquest is not yet complete; and we still find that in the very quarter in which truth

'was first lighted, the magic spell of error is not yet entirely bro-
'ken, and physicians too frequently follow its illusions.'*

"I now expressed my astonishment that the Faculty should again call up that part of my thesis, their objections to which had been previously answered; informed the Dean that as I adopted or rejected opinions, only from a belief of their correctness or falsity, I could not abandon any doctrines I had advocated, except from a conviction of their being erroneous, and requested the Faculty would grant me some time to consider their demand. This request the Dean refused, telling me I had but one alternative, either to comply or lose my degree; and leaving me with direction to wait till he could converse with the rest of the Faculty.

"After an interval of about an hour, being called before that body, I was told by the Professor of Anatomy, that the Faculty were dissatisfied with the part of my thesis previously shown to me, and insisted that it should be rescinded, and that I must immediately say yes or no. I replied, I was ready to conform to the laws of their school, if I would not be considered as in any measure abandoning my opinions. This being fairly understood, I complied with the demand; and now, to obviate any erroneous impressions as to the nature of my difficulty with the Medical Faculty, I submit to the public my essay as originally presented, and am at any moment ready to vindicate the opinions advanced therein.

J. G. W."

* "Though this is not the place to enter into a formal defence of the opinions I have advocated, I may remark, that diseases, like plants and animals, were formerly divided into classes, orders and genera, and that some of these classifications consisted of many hundred supposed varieties, each of which required some specific difference in its treatment; that the late Dr. Rush exposed the fallacy of this artificial arrangement, totally rejected the practice of prescribing for a name applied to a variable and uncertain combination of symptoms, and substituted in its place, a careful and unremitting attention to the fluctuating state of the system: and that for the promulgation of principles whose benefit to mankind is incalculable; principles which are now PRACTICALLY adopted by a vast majority of physicians in this country, and which are now daily gaining ground in Europe, he was greatly persecuted.

"During my residence in the Philadelphia Alms-House, I had an opportunity of seeing the truth of these principles experimentally proved; and as I have adopted them from a firm conviction of their being correct, cannot hesitate to ascribe honour to their distinguished author."

We have printed the obnoxious passages conspicuously, and we would beg our readers to peruse them again and again, to ascertain whether *they* can find any thing in them, deserving of so severe a sentence, as the one pronounced against them by the learned faculty; for really we must confess, that after a long and careful consideration of them, we cannot discover a shadow of objection against the doctrines they contain. The first paragraph sets forth, that it is not only dangerous, but absurd, to address our remedies to one particular symptom of disease, without reference to the immense variety of circumstances under which it may occur. We consider this excellent sense, and give the observation our most hearty concurrence. How the faculty of physic, can object to it, we are at a loss to know; but in their corporate capacity, they may have an illumination, which is refused to us plain physicians. The second paragraph is brought forward to support and confirm the maxim contained in the first. Surely the professor of the practice of physic, would not wish his colleagues to assist him, in crushing and concealing, the facts delivered by Sydenham. The third contains a beautiful comparison from Dr. Rush. The fourth states, that, when the monster Nosology reigned unopposed, a practice like the above might prevail; but that we would not expect it, when the first doctrine taught, is "to attend to the state of the system, and to prescribe accordingly." This passage being in perfect accordance with the doctrines taught in the school, we have puzzled ourselves very much to find out an objection to it. We feel not a little proud at having, as we flatter ourselves, unriddled the enigma. The professors are a worthy, well-behaved body of gentlemen, and so guarded against the use of abusive epithets, that they have felt, we conceive, angry with the young graduate for personifying Nosology, and calling him a monster! The two concluding paragraphs contain a deserved eulogium upon the late Dr. Rush. That transcendent genius, whose powers of mind, seconded by the most unremitting exertion, enabled him in a few years, to establish for America, a high and deserved medical reputation in Europe, and who, supported on the one hand by a Wistar, and on the other by a Barton, gave to the establishment, with which he was connected, a rank far above that of any other insti-

tution of his country—a rank not inferior to that possessed by any other medical university in the world.

We fear, from what we have found it our duty to write, that the faculty may suppose the bearing of our observations invidious, and, that it would be handsomer in us to allow these things to pass unnoticed. We are happy now, to have the pleasing task of vindicating the faculty, and we trust, they will allow, that if justice forces us to criticise with independence, that that justice is equally heard, when she calls on us to defend the faculty from traduction.

There are in every society, a sufficient number of idle, ill-tempered people, who give to the actions of their cotemporaries, the most unjust and uncharitable explanations. Now, this has unfortunately been the case to a very great extent, in relation to the explanation of the difficulties opposed to Dr. W.'s graduation. They not having sufficient *tact* to discover, like ourselves, the true reason in the naughty word MONSTER, have gone the length of declaring, that the single cause of difficulty consisted in an unworthy spirit of jealousy possessed by certain members of the faculty, prompting them to desire, that the name and the fame, of the immortal Rush, should be allowed to sink into oblivion. We feel satisfied that we shall be enabled to demonstrate, to the satisfaction of our readers, that this was not the case.

1st. We are persuaded that the gentlemen who compose the medical faculty, have too much good sense to suppose, for a moment, that any endeavour of their's could have the effect of blotting the name of Rush from the page of immortality.

2dly. We know that the two gentlemen of the faculty who became most conspicuous in the transaction, owe too much to Rush to come forward, and endeavour to take a just tribute of admiration from the memory of their departed friend and supporter. The Professor of anatomy, who demanded the categorical answer, yes or no, we are well assured, respects and honours the reputation of his departed teacher and patron. And although we have been told that the present Professor of the practice of physic had some disagreement with his predecessor previous to his decease, still we know, we think, enough of Dr. Chapman, to believe him above so contemptible a meanness as the one he has

been charged with. It must be confessed, that there is some difficulty in explaining, how the professor could think that there was any personality in Dr. Whilldin's observations, and in case a disagreeable suspicion might arise in the minds of some, who do not know the doctor as well as we do, we shall not rest our assertion, that that gentleman venerates the memory of Rush, on our own simple dictum; we shall bring forward unanswerable data to prove it.

It is a fact well known to those gentlemen, who were in the habit of attending the lectures of Rush, and since, of listening to the prelections of his successor, that the doctrines delivered by the former are continued to be taught by the latter, and that the most beautiful of the illustrations employed by the present professor, are literally copied from the notes of the late one. Nay more, that in some instances nearly whole lectures delivered by Dr. C. are virtually the same with some that had been read from that chair by Dr. R. We do not mention these facts with the view of finding fault with the present professor, for although we know him to be a man of very extensive abilities, still we are persuaded that the students are gainers by the plan he has adopted. Our object in bringing forward these facts, is to furnish incontrovertible proof of the gentleman's veneration for the memory of his predecessor. It is quite absurd to say in objection to Dr. C. that he delivers many of Dr. Rush's lessons without acknowledging the source from which they are drawn. When an infant draws a picture of a horse it may be necessary in order that the looker-on may ascertain what animal is designated, to write "a horse," under it; or when a man borrows an occasional idea from another, that he should acknowledge him for it. But as every one can at once discover one of Paul Potter's horses, without the name being written under it, so the Dr. in borrowing so freely and openly from Rush, very justly considers, that to name him, would be a mere work of supererogation.

Having thus fulfilled our duty to the faculty, in defending them from the unjust censures brought against them, we are again in our characters of public censors, called on to point out

fully the danger of their adopting a line of conduct so unheard of, as that, pursued in relation to Dr. Whilldin.

If we review the history of science, we observe, that in proportion as the freedom of enquiry was tolerated and encouraged, so have discoveries and improvements crowned the investigations of her votaries; that in those dark ages, when tyrants considered the ignorance of the subject necessary for the power of the ruler, science sank into a state of feebleness and decay, and, that it required the illumination and efforts of the medician family to bring into life, that expiring spark, which blazed with such lustre during the age of a Ptolemy, and an Augustus. So self-evident is this position, and so important is the well-being of science to the prosperity of the community, that the powers of Europe, although many of them are sufficiently aristocratic, have allowed to the investigations of science, the most perfect and unconstrained latitude of enquiry. We are satisfied that we are supported by truth, when we make the assertion that the Faculty of Physic of the Pennsylvania University, have in their conduct to Dr. Whilldin, employed a stretch of power without a parallel. In the former part of the enquiry, we have, for the purpose of entering into a clear investigation of the subject, taken it for granted, that such a power as that exercised by the professors was really within the rule of their office. We have been unable in this view, to discover that the matter complained of, could, by the most learned, be considered destitute of good sense, or by the most squeamish as tinctured with odious personality. But we shall now go farther and suppose, that Dr. Whilldin, instead of eulogising the late Dr. Rush, had thought fit either to criticise Dr. Physick's animal ligatures, or Dr. Chapman's theory of generation; we aver that had he even done so, neither of these gentlemen were, in their professional characters, entitled to offer a single objection against the reception of his thesis. They might have demanded from him a defence of his opinions; but this being given in a spirit of rationality, they were in duty bound to award to him the honour, which he had a right to look for.

Should the system pursued by the faculty in this instance be continued, there is a chilling influence thrown over the investiga-

tions of the student. If he must measure the scale of his mind by the line of his teacher, what are we to expect of him?—A commentary on one of the new MEDICAL CLASSICS!! Had the mind of Harvey continued to bind itself to the dogmas of Galen and Avicenna, where would have been the discovery of the circulation of the blood? or if the genius of Columbus had allowed itself to continue fettered by the narrow and confined views of the navigators of his day, what would now be America?

Genius is a plant of the most delicate growth; one which requires the utmost care and attention in its culture. If darkened and crushed when it first seeks the light, it is more than probable that its progress will be feeble and unenergetic. But if it be cherished and supported when it first appears, by the countenance of a respected teacher, it will take root, and " its fruit will be for the healing of the nations." Could any treatment have a more deadening influence on a young man just entering into life, than that pursued towards Dr. Whilldin. " The faculty," says a professor, " are dissatisfied with a passage in your thesis; we shall not reason with you, as to whether you are right or wrong, tear out this disagreeable passage and we shall give you your diploma; refuse to do so, and we shall withold it." We are only sorry that Dr. Whilldin was not better advised, and that he did not boldly refuse to comply with a request so inconsistent with the laws of liberality, of justice, and of his country. We would not wish to be misunderstood here; we are among the last living who desire to encourage a spirit of disorder in the student. We venerate, and so far as we have the power, shall support the privileges and the character of the conscientious professor. We duly estimate the character, and shall ever plead for the rights of the diligent and attentive student.

That our views may be perfectly understood in relation to these two characters, we shall endeavour in a few words to portray the features of each. The professor, who is anxious to obtain the love and respect of his pupils, must devote his whole heart and soul to the department he teaches. His chief delight must consist in acquiring and conveying information; he must teach for the love of teaching, not for the love of gain. To his students he

must be kind and attentive, frank, but not familiar. They will soon discover how anxious he is to promote their improvement, and the kindlier feelings of the one party, will bring into action those of the other. The interests of both become amalgamated, and the compact of benefit being mutual, harmony is the necessary result. When a professor fulfils his part of the contract, he does not require to be endowed with any restrictive power over the mind and opinions of the student.

If a man however in the high character of a professor does not fulfil his duties, he ought not to calculate on the respect and attachment of his pupils. If lecturing is to him a task which he is glad to hurry over, one which he performs merely for the consideration of money; if he is irregular in his attendance, and instead of appearing exactly at his hour in his class-room, he comes in late, and after a desultory half hour's conversation dismisses his pupils discontented; if he is so ill informed on the subject he professes, as to require to have noted on a scrap of paper, the doses of the simplest medical substances, the names of the processes of the bones, or the simplest combination of the acids, and alkalies; if he does all this, he ought neither to be beloved nor respected.

There is no character more to be estimated than that of the diligent and attentive student. The young man who pursues his professional enquiries with ardour and enthusiasm, who listens to the prelections of his professor with attention and respect, who examines with care all he hears, and reserves, under the control of a just circumspection, the privilege of examining facts, and judging with modesty for himself; such a man is in his character as honourable, and ought to be as cherished a character as his professor. Let it ever be remembered that the professor was once the student.

The duties of the physician are the most interesting to humanity. The tenderest and most cherished ties of society are in his hands, and upon his skill or ignorance, these are either to be strengthened or broken. Every man can form a pretty just estimate of the abilities and character of his pastor, or of the genius and talent of his lawyer. No man but a physician is qua-

lified to judge of the skill and medical learning of a physician. As the character of the medical practitioner is, therefore, one of such importance in society, and one which it is so difficult justly to judge and estimate, we conceive that no branch of medical police should be more carefully watched and guarded by the public censors, than that of granting diplomas. Colleges are erected by the legislature for examining young men, and declaring whether they are entitled to public confidence; we allow that under the best arrangement ignorance will frequently pass this ordeal; but the knowledge of this fact, merely operates as an incentive to us, in searching out, and exposing every departure from duty in the conduct of those who are entrusted with the guardianship of the public safety. We confess that we are further incited to notice, in terms of severe reprehension, the system of preparation which has of late gone into extensive usage in our university; for the connection which *in after life subsists* between an *alumnus* and his alma mater is of a kind and filial character. He naturally considers her fame and reputation in a manner as interwoven with his own; and holding, as he does, his diploma from her, he correctly judges that the respectability of this his certificate of qualification, will vary in the honour it bestows, just as the institution from which he holds it rises or sinks in the scale of reputation.

Children as we are, of the University of Pennsylvania, we have with the most anxious solicitude watched her for the last few years. With grief and despair we have seen her deprived of her Rush, her Barton and her Wistar. We felt persuaded that such losses were not to be replaced, and under such an assurance she has had our most tender sympathy. We do not propose now to enter into the enquiry whether the chairs be filled judiciously, or what exertions have been made with the view of balancing the loss of talent? Such queries, however interesting and important they may be, we wave for the present, as being foreign to the avowed object of the enquiry. With the subject of graduation alone do we intend to occupy ourselves.

So far as our information goes, we are prepared to grant that it has been customary, both in the medical institutions of this coun-

try and in those of Europe, for the *graduates elect*, to prepare themselves for their examinations, by a course of questionings on the topics which were probably to be enquired of them by their professors. Under due restrictions, we have no great objections to such a preparatory exercise; for, although we had much rather see students prepare themselves, by a full and extended study of the different branches of the science, still we are willing to allow, that for the sake of the timid and fearful, the grinding system of Edinburgh, or the quizzing one pursued by the students themselves, may offer some advantages.

As all of our readers may not be aware of what we mean by the terms *grinding* and *quizzing*, we shall for their sakes explain in a few words what is understood by these two terms.

An Edinburgh Grinder is generally one of those unfortunate young men called *sticked ministers*, who during the term of their study for the ministry have allowed, in some unlucky moment, the organ of *l'Amour Physique* to overrule that of theosophy; in simple language, one of those who become the parent of an offspring without any other title than a natural one. Unfitted by these *faux pas* for the service of the kirk, they are obliged to draw on their classical acquirments for the means of present support, and no plan is more likely to succeed, than that, of going to Edinburgh and becoming grinders. That is to say, to go about the medical classes, and for a certain consideration, to meet the students privately and converse with them in the Latin language, on the subjects which they will probably be examined on hereafter, by the professors.

The quizzing system formerly pursued, in the school of medicine of Pennsylvania, was confined altogether to the students. Small parties associating themselves together, met and spent an occasional hour, in questioning each other. We again repeat, that all this is very well, and so long as it is confined to the grinder or the student, we have no fault to find with preparatory exercise.

When, however, the professor descends from the rank and dignity which his situation confers upon him, and condescends, for the sake of an enormous fee, to become the grinder or the

quizzer of his student, we can no longer remain silent; our every interest, public and private, calls upon us imperatively, to enter our protest against a proceeding, which will more effectually tend to undermine and ruin the reputation of our alma mater, than all those injudicious arrangements, which have been of late years adopted.

We think much too highly of certain of the professors who have gone into the quizzing Business, to believe that they could possibly be aware of the baneful and ruinous tendency of the system which they have adopted, and we feel almost persuaded, that so soon as they have perused this essay, they will thank us for opening their eyes to a true view of the fearful consequences of the plan they have inadvertently followed.

That the effects of this preparatory system may be fully exposed we shall, in a few words, show the tendency of such a quizzing exercise. The professor is the quizzer, his pupils are the quizzed. The individual, who is afterwards to decide upon the qualifications of the candidate for a diploma, is the same person who, for a year previous to it, is in the constant daily practice, of enquiring of him, answers to those very questions, which are afterwards to be proposed to him, as a test of his qualification for the highest honour in medicine. Again, more than one professor associates in the same concern; in plain undisguised language, a copartnership is formed, for making Doctors, in which the divisions of the profits are as well calculated as the nicest settlings on the exchange. We can find no apology for this money-making system. The professors in this university have been in the habit, of late years, of receiving, from their chairs, a remuneration in money superior to that received by any other body of men in the same situation. For four months teaching, these fees, have for some years past, ranged annually from $8,000 to $10,000.

It must become self-evident that from one year's quizzing, the most trifling and deficient student will acquire from this continued drilling, a parrot-like habit of giving answers to questions; concerning the spirit, the science, and the philosophy of which, he remains in the darkest and most disgraceful ignorance. Are we, who laboured incessantly during the term of our studies; to

be placed on the same rank with every driveller, who can afford to fee this company? God forbid!

If it should be determined, that money is to purchase that, which ought only to be awarded to diligence and exertion, we would propose that the farce of an examination should be wholly dispensed with, and that degrees should be sold wholesale and retail after the fashion of the schools of Aberdeen and St. Andrew's. There is a stern, plain simplicity adopted by these ancient Scottish seminaries, which we cannot help admiring. They do not attempt to varnish over the matter, but with perfect candour declare, that they will make doctors for the small charge of 24*l.* 5*s.* 3½*d.*; this is perfectly open and candid, and as we cannot see any difference betwixt giving a diploma, without any examination, and granting one to a candidate, for answering questions he has been drilled into for a year, we would, if the quizzing system should be allowed to continue, seriously advise, that the plan of Aberdeen be adopted "in toto." It will save the students much time and money, and will bring into the general coffer, that which under the present arrangement falls only to a portion of the body corporate, and may then go towards buying books or founding a museum.

As we should wish to anticipate any objection which may be attempted against the consistency of our observations, we would observe, that a professor having private pupils, and holding a quizzing class, are two very different things. We allow that those great men whom we have mentioned with so much respect were in the habit of having private pupils. Yet the knowledge of this fact, takes in our estimation nothing from their reputation. To bestow medical instruction upon a limited number of young gentlemen, and to allow them the benefit of colloquial information, is perfectly in tone with the rank and character of a professor. But to become a quizzer, to receive $100, from fifty or sixty students for drilling them to answer questions, is really and truly to quiz the pupil, and to tend to degrade the high standing of the professor, to the same level as that of the "*sticked minister.*"

One caution and we have done; as the spirit and bearing of all our observations are intended merely as correctives, we would

wish to convey no personalities, and have therefore wrote as generally as the nature of the case would admit. We are aware that our generalizing may be taken amiss by certain members of the faculty, who feeling equally with ourselves the injurious tendency of the system of quizzing, give to it their most decided disapprobation. It is an unfortunate, although a necessary evil, that when there is any variation from correct thinking or acting in one or more members of a body corporate, the blame is thrown not on the individual, but on the corporation. We sincerely hope, however, that these our observations will be taken in the spirit in which they are intended. When we observe the great strength which rival institutions on each side of us are acquiring, we tremble for our alma mater. We would wish to purge her of every thing which can derogate from the respectability of her diplomas, and to open the eyes of all her professors to the fact, that it will require from them great exertions to retain the standing which this school now holds. Let them not, we beseech of them, continue to deceive themselves by applying this flattering unction to their minds, that, as the school of Pennsylvania was once the great medical establishment of the United States, it must for ever remain so. It was their Woodhouse, their Barton, their Rush, and their Wistar, that gave to it this high and elevated standing, and although their great and deserved reputation may have thrown a halo around it, which it will require some time to dissipate, still the hour is rapidly approaching when the university of Philadelphia, must be measured by that talent which she possesses, not by that which she could formerly number.

To the Editor of the American Medical Recorder of Original Papers and Intelligence.

Sir,—In the number of the work above mentioned for July 1819, you employ the following language, in speaking of the calorimotor, a galvanic instrument of which I had published an account a short time before: "This instrument appears to us,

nothing more than an extension of the elementary battery of Wollaston."

Subsequently, in conversation, I pointed out the injustice of this treatment. You admitted the propriety of my animadversions, and promised to correct your error, alleging your own incapacity to judge of the subject, and the necessity you had been under of adopting the impressions of others whom you had considered as judges. It seems however that you have concluded not only to reiterate in the Recorder for April, the opinion above mentioned, but to support it by statements which I shall prove could never have been made sincerely, had you understood the authors to whom you refer.

In the first place I must observe, that the calorimotor as represented in the plate annexed to my memoir, consists of *four galvanic surfaces*, or *two pairs*,* with *one recipient*. I assert, that you cannot adduce any *analogous contrivance*, *small* or *great*, of *Dr. Wollaston* or *any other person*. Such a combination of *uninsulated* surfaces, would not probably have occurred to any one deeming galvanism to be electricity under a slight modification; as that ingenious philosopher has supposed and endeavoured to prove. I should have hoped that before criticising my pamphlet, you would at least have examined the engravings which accompany it. A much more vivacious and intense ignition may be produced, by two large galvanic pairs situated as I have arranged them for one recipient, than could be excited by double the extent of surface in one pair formed like Wollaston's. After my experiments with one large pair were known, Mr. Wetherill, expecting to produce more striking or powerful results, made a pair of double the size. By this, a wire of platina or iron, whether large or small, could not be heated much more than red hot; though similar wires by my smaller apparatus of two uninsulated pairs, were rapidly fused or burned off. Mr. R. Peale having also made an apparatus of one pair more extensive than mine, was disappointed as Mr. Wetherill had been.

I advised him to separate the two surfaces into four surfaces alternating. When thus altered, platina or iron duly exposed to

* See plate and description.

their action, were rapidly thrown into a state of fusion or combustion.

I will next observe, that the action of a pair of galvanic surfaces was known not only before the time of Wollaston, but before that of Galvani himself. The taste excited by putting two plates of different kinds of metals one above, and the other beneath the tongue, and bringing into contact, their projecting extremities, was just as much the effect of a galvanic discharge as the ignition produced by the elementary battery. Hence it must be evident that it is only in size, arrangement, and application that this last mentioned apparatus can possess originality. But in all these points I trust that the characteristics of the calorimotor are at least equally peculiar.

Yet as Wollaston constructed a minute galvanic pair *for producing* ignition, and I constructed a pair enormously large by which ignition *may be produced;* you seem to infer that the one must be an extension of the other. It did not occur to you, that both, might be opposite varieties of a contrivance previously existing. Had the novelty of Wollaston's battery been dependent on the isolated fact of its producing ignition, your remarks had been more justifiable. But it was nothing new to produce galvanic ignition, and from his account of the elementary battery which I shall presently quote, it does not appear that he considered the result as meriting attention independently of the *minuteness of the means* by which it had been accomplished. It is not to the fact simply of his *igniting wire* that he attaches importance, but to the circumstance of his having *ignited it by an apparatus almost inconceivably small.* To use his own words, the purpose of his experiments was "to ascertain the most compendious form of apparatus by which visible ignition might be produced."

I am aware that with plates of a moderate size he made an instrument for producing instantaneous light, but having neither seen any account of that contrivance in the scientific journals, nor heard of any, I am doubtful whether it was considered as new, unless in the arrangement or application. Even this enlargement of his battery destroyed the most striking, and what he seems to have deemed the most important distinction, between it

and an ordinary pair: and admitting that he were the first to employ such a pair for the purpose of ignition, there had on that account been no more reason for representing him to be the inventor of this most simple and most ancient galvanic combination, than for attributing the invention of the Voltaic pile, to Carlile and Nicholson, because of their priority in applying it to the decomposition of water. Granting therefore that the ignition which I produced, was nothing but an extension of that which he had before effected, it does not follow that my apparatus was nothing but an extension of his; nor that there may not be as much originality in the enlargement of the original galvanic pair, under one form, for the purpose of investigation, as in the diminution of it under another form, for the same object.

But unfortunately for the correctness of your strictures, you seem to test my pretensions to experimental invention by one result of my contrivance, totally overlooking that, by which charcoal was shown to be impermeable to the galvanic fluid, as extricated by the calorimotor. The experiment by which this was proved, is thus mentioned in my memoir:

"The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

"Thus prepared, the interior cylinder being made to touch one of the galvanic surfaces, a wire brought from the other galvanic surface into contact with the outside cylinder, was not affected in

the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal, with the containing metals probably took place throughout a superficies of four square inches, and the wire was not much more than the hundreth part of an inch thick, so that unless it were to conduct electricity about forty thousand times better than the charcoal, it ought to have been heated; if the calorific influence of this apparatus result from electrical excitement."

To meet you, however, on the very ground which you have chosen, I must first quote Dr. Bostock's account of the experiment, of which, it would seem, you consider mine to be an extension; and the passage in my memoir, in which the latter is described.

"Dr. Wollaston constructed a very curious apparatus, which he calls an elementary galvanic battery, the *object* of which is to exhibit the *most minute* arrangement of *electrical* substances, by which *visible* ignition can be produced. The *smallest* that he has constructed, consists of a thimble without its top, flattened, until its sides were about one-fifth of an inch asunder; a small plate of zinc was then contrived to be fixed within the thimble, but without touching it, and a proper appendage of platina wire was added. The zinc plate was less than three-fourths of an inch square, and even when water was employed that contained one-fiftieth part of sulphuric acid, a platina wire of one three-thousandth part of an inch in diameter, and from one-thirtieth to one-fiftieth of an inch in length, was readily fused."

The passage in my memoir above alluded to, is as follows: "I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of interposed agents lessened, the ratio of electrical effects to those of heat had increased till in De Luc's column they had become completely predominant; and on the other hand, when the pairs were made larger and fewer, (as in Children's apparatus) the energy of interposed agents not being lessened, the calorific influence had gained the ascendency. I was led to go further in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it equally with the electric fluid a primary product of galvanic combinations.

The elementary battery of Wollaston, though productive of an evanescent ignition; was too minute to allow him to make the observations which I had in view."

How can an experiment designed for the examination of the heat moving powers of the *largest* galvanic pair ever made, be considered as an extension of another, which has for its "object, the most minute arrangement of electrical substances, by which visible ignition can be produced?" Besides, an extension of the generating surfaces alters the character of the ignition. The phenomena of the minute battery of Wollaston, appear much more liable to be mistaken for those of electricity, than the sluggish movements of the calorific principle, as exhibited in a galvanic pair of the largest size. By such an apparatus, though inadequate to fuse a very small platina wire, I have seen a strip of tin plate, nearly four inches broad, kept at a gentle heat for a considerable time.

Every galvanic observation or modification, subsequent to the use of the original elementary pair, may be said to have grown out of that contrivance. By multiplying such combinations, the agency of electricity in galvanism, was displayed. By lessening the original pair, Wollaston ascertained the curious result, that even when *very minute*, ignition may be effected by it. By enlarging it, I sought to show the phenomena inconsistent, with the idea that the ignition resulted merely from an electrical discharge. Commencing both from the same original point, we proceeded in opposite directions, and with different designs. The fact, that ignition might be produced by galvanism, having been long known, it was, in the examination of the characteristics of this ignition, with a view to the nature of its cause, that further elucidation was expected by me. Assuming the cause to be electricity, he neither suited his apparatus, nor directed his thoughts to such an investigation; while my apparatus, expressly designed for this purpose, by magnifying results, made them more susceptible of scrutiny, and using charcoal as a test, I availed myself of the opportunity to demonstrate the existence of a decisive difference between the calorific agent in galvanism, and the fluid with which it had been identified by him. It were then very unfair to convey the idea, that my experimental enquiry was

nothing more than an extension of his. I have already shown, that this notion, could not, with propriety, be conveyed, with respect to my apparatus.

Moreover it will be admitted that experiments derive much of their value from the hypothetical considerations, by which they are induced, or with which they are associated. You have described the theory which I have advanced as "new, plausible, and ingenious."

In the following passage in my memoir, it is briefly stated:

"According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air with immeasurable celerity, and distributing itself through the interior of bodies, communicates a reciprocally repellent power, to atoms but not to masses. Electricity does not radiate in or through any matter, and while it pervades some bodies as metals with almost infinite velocity; by others it is so far from being conducted that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes reaction between masses, but not between the particles of the same mass. The disposition of the last mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties consequent to some such union, they could not, in piles of thousands of pairs, be carried forwards through the open air and moisture, the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

"Pure electricity does not expand the slips of gold leaf between which it causes repulsion, nor does caloric cause any repulsion between the ignited masses which it expands. But the compound fluid extricated by galvanic action which I shall call electro-caloric distributes itself through the interior of bodies, and is evidently productive of corpuscular repulsion, it is in this respect more allied to caloric than to electricity.

"It is true that when common electricity causes the deflagration of metals, as by the discharge of a Leyden jar, it must be supposed to insinuate itself within them, and to produce a reaction between their particles. But in this case, agreeably to my hypothesis the electric fluid combines with the latent caloric previously existing there, and adding to its repulsive agency, causes it to overpower cohesion."

Agreeably to this doctrine, the mysterious difference between galvanism and electricity is explained, by supposing the former, the result of the joint action of caloric and the electric fluid, their ratio to each other in quantity varying according to the apparatus used to produce them. In De Luc's column, the electric fluid is presumed to be at a maximum, the caloric of course at a minimum; the phenomena being apparently those of mechanical electricity, though the means of production are galvanic.

In the pile of Volta, or the troughs of Cruickshank and others, both fluids are conceived to be active, the ratio of their quantities to each other according to the law, stated in the passage already quoted.

To complete the illustration, a third form of apparatus was requisite, in which caloric should be at a maximum, electricity at a minimum.

Your animadversions in the Recorder for April, are obviously intended to promulgate the impression that Children's Battery and Wollaston's had left nothing further to be desired. But as the characteristics of De Luc's column are, that the size of the galvanic surfaces shall be very diminutive, their number very great; to attain the opposite extreme, it was necessary to seek the smallest number, and the largest size that could be made to avail. Children's apparatus wanted the former characteristic; Wollaston's the latter; and though I was the first to supply this deficiency, although my theory of galvanism which you call "plausible and ingenious," is as far as *you know* new, and it must be evident that the calorimotor grew up with, and had become a necessary prop to that hypothesis; you allege "If Professor H. therefore, wished to produce a very powerful evolution of heat without a concomitant production of electrical phenomena, his course of

proceeding with a knowledge of the above facts was at once simple and obvious."

As even the discovery of the new world appeared simple and obvious to some persons after it had been accomplished by the great Columbus, an author must be monstrously vain and madly sanguine, who expects that his performances may not be viewed in the same way. The language you have here adopted might be employed to disparage discoveries which have gained the highest applause. You might have said when Nicholson and Carlile had effected the decomposition of water by the Voltaic pile, if Sir H. Davy wished to decompose substances in which the elements were more forcibly combined, "his course was at once simple and obvious," he had only to use an apparatus as much more extensive, as the decomposition was more difficult. If Dr. Black wished to ascertain whether any, and what principles were extricated during the calcination of lime-stone, "his course was at once simple and obvious," he had only to heat it in a retort, and catch the products. After a peculiar gas had thus been discovered in lime-stone, Dr. Priestley being desirous to learn whether there were not other kinds of gas in nature, "his course was at once simple and obvious;" he had only to subject other substances to the same process. When Watt had perfected the steam engine, and boats had been moved by paddles, if Fulton wished to propel them by steam, "his course was at once simple and obvious."

The galvanic fluid as generated by Children's apparatus, permeates and ignites charcoal. It is therefore a galvanic instrument of the middle class, and not analogous to the calorimotor. The vivacity of the ignition in Wollaston's battery may awaken some doubt whether the disproportion between the component principles of the galvanic fluid, as excited by it, is at a maximum as in the case of a very large pair like mine. Hence the contrivances of these eminent chemists are not only in form unsuitable to be classed in the extreme, the opposite of that, in which we find De Luc's column; but the phenomena do not warrant such a classification.

I adverted in my memoir to Wollaston's Elementary Battery, and the ignition produced by it; when stating the fact of my having

made, for the purpose of investigation, a larger pair than any used before. Independently of the peculiarities of such a pair, already mentioned, there is in the distribution of the surfaces, a novelty which may be better estimated by turning to the latter part of the paragraph already quoted from Bostock. He says, speaking of Wollaston: "In the formation of his minute apparatus this sagacious experimentalist made an observation of great importance in enabling us to produce as much effect as possible from the smallest quantity of materials. He remarks that the zinc plate should have a counterpart of copper opposed to each side of it; for in the usual arrangement, although both sides of the zinc are oxidated, that side only is efficient which has a copper surface opposed to it." Agreeably to this arrangement *so very highly commended*, there must be *twice as much copper as zinc*. In my apparatus, *every zinc surface has its counterpart of copper excepting the last;* though *the quantity of the metals is the same*. I have reasons in addition to these, for believing I have adopted a more efficient mode of using the same extent of surface, than had been devised before.

In support of the following passage you adduce *no authority*, I aver it to be a statement not only *groundless*, but contradictory to *Wollaston's own words*, to *facts* that are *well known*, and to your own admission that my theory is as far as you know new. You say, "in conformity with those principles, Dr. Wollaston constructed an apparatus of two plates only, and found that by thus lessening the number of the plates, although very small in size, heat was powerfully evolved without a proportional production of electrical effect." So far is Dr. Wollaston from distinguishing the heat produced by his apparatus from the electrical effect, that he considers it an electrical effect. He is the author of an experiment to show that the galvanic, and electric fluids are the same, the latter requiring to be emitted from an impalpable point. Had he made the distinction you allege, he had anticipated my hypothesis; the fundamental basis of which is, that the ignition of wire, by the calorimotor results from a current of caloric, not of electricity. The diffidence which qualifies your commendations with the words, "as far as I know," would never have

operated with better advantage to yourself than when writing the above mentioned account of Wollaston's hypothetical inferences. Let Wollaston speak for himself. Annals of Philosophy, vol. 6. page 209. He thus expresses himself in a letter to the editor:

"DEAR SIR—I now send you a description of a *small battery* which I shewed you some time since, and shall feel obliged by the insertion of it in your Annals. Since the ignition of metallic wires is rather instructive with respect to the vast *quantity of electricity* evolved during the solution of metal, I made three years since a series of experiments for the purpose of ascertaining the most compendious form of apparatus by which visible ignition might be shewn.

"The result of these trials was, that a single plate of zinc one inch square, when rightly mounted, is more than sufficient to ignite a wire of platina even when the acid employed is very dilute."

He says again in the last paragraph of the same letter, "Although in this description I have mentioned a wire $\frac{1}{1}$ of an inch in diameter, I am doubtful whether this thickness is the best, I am however pursuaded that nothing is gained by using a finer wire; for though the quantity of matter to be heated, be thus lessened, the surface by which it is cooled does not diminish in the same ratio, so that when the cooling power of the surrounding atmosphere is the principal obstacle to ignition, a thicker wire which *conveys more electricity* in proportion to its cooling surface, will be more heated than a thin one, a fact which I not only ascertained by trials on these minute wires, but afterwards took occasion to *confirm* on the largest scale by means of the *magnificent battery* of *Mr. Children* in the summer of 1813."

Let me next cite the impressions of an editor, who does not write, on what he does not understand. "Dr Wollaston's elementary galvanic battery described in a late number of the *Annals of Philosophy*, constitutes a discovery of considerable importance. It demonstrates the quantity of electricity which is disengaged during the chemical action of acids on metals, and thereby serves to throw much additional light upon the still obscure

theory of galvanism."* It is unnecessary to state the only deduction which can arise from a comparison of these quotations, with that above made from your critique.

The following statement is equally unfounded: "Thomsdorf, Thenard, Fourcroy, Vauquelin, and particularly Children, have demonstrated that the *electrical* action is not increased by the increase of the size of the plates, but by the increase of the numbers of pairs, and that on the contrary the evolution of heat is increased with the size of the plates."

Since the simple galvanic combination of Wollaston was supposed to afford instruction by the quantity of *electricity* given out, more complex combinations which by your own premises are more highly electrical, could not but be viewed in the same way.

The reason given by Dr. Wollaston in the passage above quoted, why a large wire will be more easily heated than a small one is, that it "conveys more electricity," and he has been confirmed in this explanation "by means of the magnificient battery of Mr. Children."

Is it not then evident that he considered electricity as the common cause of the heat in both his battery, and that of Children. If evidence be required of Children's opinion, let us turn to Dr. Thomson's account of this battery. In *Annals of Philosophy*, vol. 7. p. 11. he tells us, "The order in which metallic wires connecting the two poles of this battery became red hot, was as follows:

Platinum	Copper	Zinc
Iron	Gold	Silver

"*Mr. Children conceives, that the metals conveying electricity, became red hot inversely as their conducting power.*" Sir Humphrey Davy, in mentioning a large battery of Mr. Children, says "This battery, when in full action, had no more effect on water or on the human body, than one containing an equal number of small plates; but when the circuit was made through metallic wires, the phenomena were of the most brilliant kind."† The rationale of this difference is, according to the same author, that

* Thomson's Annals. † Davy's Elements, p. 84.

"the quantity in the small plates is as much, or more, than such imperfect conductors as water, and the human body can carry off by a small surface, whilst better conductors, can transmit the whole quantity afforded by the large plates, even when used in very thin lamina or wires."*

From these passages it must be evident that Sir H. Davy considers both the ignition produced by a few large pairs, and the shock or decomposition resulting from many small ones, as the effect of the same electric fluid, in quantity greater in the first case, in the last more intense. It will be found on consulting Thenard, that he has no other conception, than that all the effects of the pile result from the electricity which it generates or puts in motion.

It is then undeniable, that the very authors whom you erroneously cite, as having demonstrated that heat might be produced by galvanic apparatus without a proportional production of electrical effect, really consider the heat thus produced, as an electrical effect, and of course, had not discovered it to be otherwise. Hence, correcting one of your quotations, it would read thus: "Heat and electrical effect, was evolved without a proportional production of electrical effect!!!"

Experimental novelties have a two-fold value; first, as they add to that captivating splendour to which science owes so much of its power, in affording innocent and rational amusement; and secondly, as they tend to create new distinctions, either between facts or principles, previously confounded. Those who have witnessed the combustion of wire by the calorimotor in its improved state of two uninsulated pairs, with the attendant phenomena of the inflamed hydrogen, allege that it affords one of the most splendid spectacles in the chemical course, especially when compared with the simplicity of the process, and the facility of repetition. I submit it to my pupils to say how far any experiment with a *thimble*, and an almost inconceivably small wire, can ever, in this respect, come in competition with those which I showed them by means of the instrument in question. According to the second view of the subject, I deem it of more importance to have ascertained, that a

* Traite de Chimie.

large galvanic pair, or even two uninsulated pairs, *will not ignite charcoal*, than that a small pair *will ignite wire.* The first mentioned experiment distinguishes the fluid put in motion by one or two uninsulated pairs, from that evolved by the Voltaic pile, which the latter does not.*

My theory of galvanism has been republished in some of the most respectable journals in Europe, as well as in this country, without any idea being excited, that I have not, as you would insinuate, "given unto Cæsar, the things that are Cæsar's;" an injunction originally intended to sanction the payment of a tribute actually required by Cæsar, and in a coin carefully ascertained by competent judges to bear his stamp. But it was not designed to authorize the officious and unskilful to render unto Cæsar, things, which Cæsar had not demanded of them, which he would despise as coming from them, and which might, for aught they could judge, be the property of another.

Wollaston is too well fortified by his splendid genius and extensive acquirements; too well surrounded by his scientific compeers, to require the aid of your eloquence or erudition, to guard him from the consequences of any wrong which I have the power or disposition to commit. And I doubt, if your readers will feel themselves indebted to you, for deviating from the obvious duty of a recorder of original papers and intelligence, in order to communicate the opinions or misconceptions of yourself or your advisers, on the subject of my calorimotor.

* For the information of readers who may not have witnessed this spectacle, I beg leave to state, that by the time the calorimotor is completely immerged in the acid solution, the wire in the forceps is rendered white hot, and takes fire, emitting the most brilliant sparks. In the interim, an explosion usually gives notice of the extrication of hydrogen in a quantity adequate to reach the burning wire. Immediately after the explosion, the hydrogen is reproduced with less intermixture of air, and rekindles, corruscating from among the twenty interstices, and passing from one side of the machine to the other, in opposite directions and at various times, so that the combinations are innumerable. The flame assumes various hues, from the solution of more or less of the metals, and a froth apparently on fire, rolls over the sides of the recipient. When the calorimotor is withdrawn from the acid solution, the surface of this fluid for many seconds, presents a sheet of fiery foam.

Since writing the above, I find from the following observations of the celebrated Dr. Thompson, that *he* considers my apparatus as a "new modification of the galvanic battery."

"*Galvanism.*—Dr. Hare, Professor of Chemistry in the Medical Department of the University of Pennsylvania, has published (in a paper to be found in the *Annals of Philosophy*, xiv. 176,) a theory of galvanism differing considerably from all those hitherto started. According to him, the galvanic fluid is a compound of caloric and electricity. The electricity is increased by the number of pairs of plates, and when this number is very great, as in De Luc's column, the calorific effects become evanescent. The caloric is evolved by the increase of the surface, and he has shown that it may be very intense, when only a single pair of plates, or what is equivalent to it, is used. He has given us a drawing of a galvanic battery constructed on this principle, which produces intense ignition without any electrical phenomena.

"As I have not yet had an opportunity to study the phenomena exhibited by this new modification of the galvanic battery, I should consider it as improper to make any remarks on Dr. Hare's ingenious hypothesis at present. What I have to say on the subject, therefore, I shall reserve for another opportunity."—*Thomson's Annals of Philosophy.*

EXPLANATION OF THE PLATE.

A *a*, Fig. 1st, two cubical vessels, 20 inches square, inside. *b b b b* a frame of wood containing 20 sheets of copper, and 20 sheets of zinc, alternating with each other, and about half an inch apart. T T *t t* masses of tin cast over the protruding edges of the sheets which are to communicate with each other. Fig. 2, represents the mode in which the junction between the various sheets and tin masses is effected. Between the letters *z z*, the zinc only is in contact with the tin masses. Between *c c* the copper alone touches. It may be observed, that, at the back of the frame, ten sheets of copper between *c c*, and ten sheets of zinc between *z z*, are made to communicate, by a common mass of tin extending the whole length of the frame, between T T: but in front, as in Fig. 1, there is an interstice between the mass of tin connecting the ten copper sheets, and that connecting the ten zinc sheets. The screw forceps, appertaining to each of the tin masses, may be seen on either side of the interstice: and likewise a wire for ignition held between them. The application of the rope, pulley, and weights, is obvious. The swivel at S permits the frame to be swung round and lowered into water in the vessel *a*, to wash off the acid, which, after immersion in the other vessel,

might continue to act on the sheets, encrusting them with oxide. Between *p p* there is a wooden partition which is not necessary, though it may be beneficial.

Fig. 3, represents an apparatus alluded to, page 419.* It consists of a couronne des tasses, reduced to a form no less compact than that of the trough. Hollow parallelopipeds of glass are substituted for tumblers or cells. The plates are suspended to bars counterpoised like window-sashes.

The advantages are as follows. The material is one of the best non-conductors, is easily cleansed, and is the most impervious to solvents. The fracture of one of the cups is easily remedied by a supernumerary. They may be procured (as in the United States) where porcelain cannot be had. The shock from 300 pairs is such as few will take a second time. Some of the effects have already been stated.†

At Fig. 4, one of the hollow glass parallelopipeds on an enlarged scale is represented.

Remarks by Dr. Eberle.

I HAVE not erased a single word from the above reply, that the offended professor, may not imagine that I deprecate the severity of his animadversions, any more than I dread the force and acuteness of his reasoning.

The statement given by the professor, relative to the conversation that passed between us, is, I explicitly aver, erroneous, as to its import. I would not, however, be thought to believe, or wish to insinuate, that the misrepresentation arose from design. I am certain, that this cannot have been the case. Soon after the publication of my remarks, on his calorimotor, he accosted me, in nearly the following words: "If the person, who wrote the remarks on my calorimotor, published in the last number of the Recorder, had wished to do me justice, he would have quoted this passage," (at the same time pointing out the passage alluded to,) "instead of the one, which he did quote." I replied, that I wrote the article myself; and, in order to let him know, that the opinion, I expressed in that article, is entertained also by others, as competent at least, to give an opinion upon subjects of this kind as himself, I stated, that not wishing to rely wholly on my

* Of Vol. II. Hen. Chemistry.

† The glasses may be had by applying to Edw. A. Pearson, No. 71 Cornhill, Boston.

INCUBATOR.

Fig. 1.

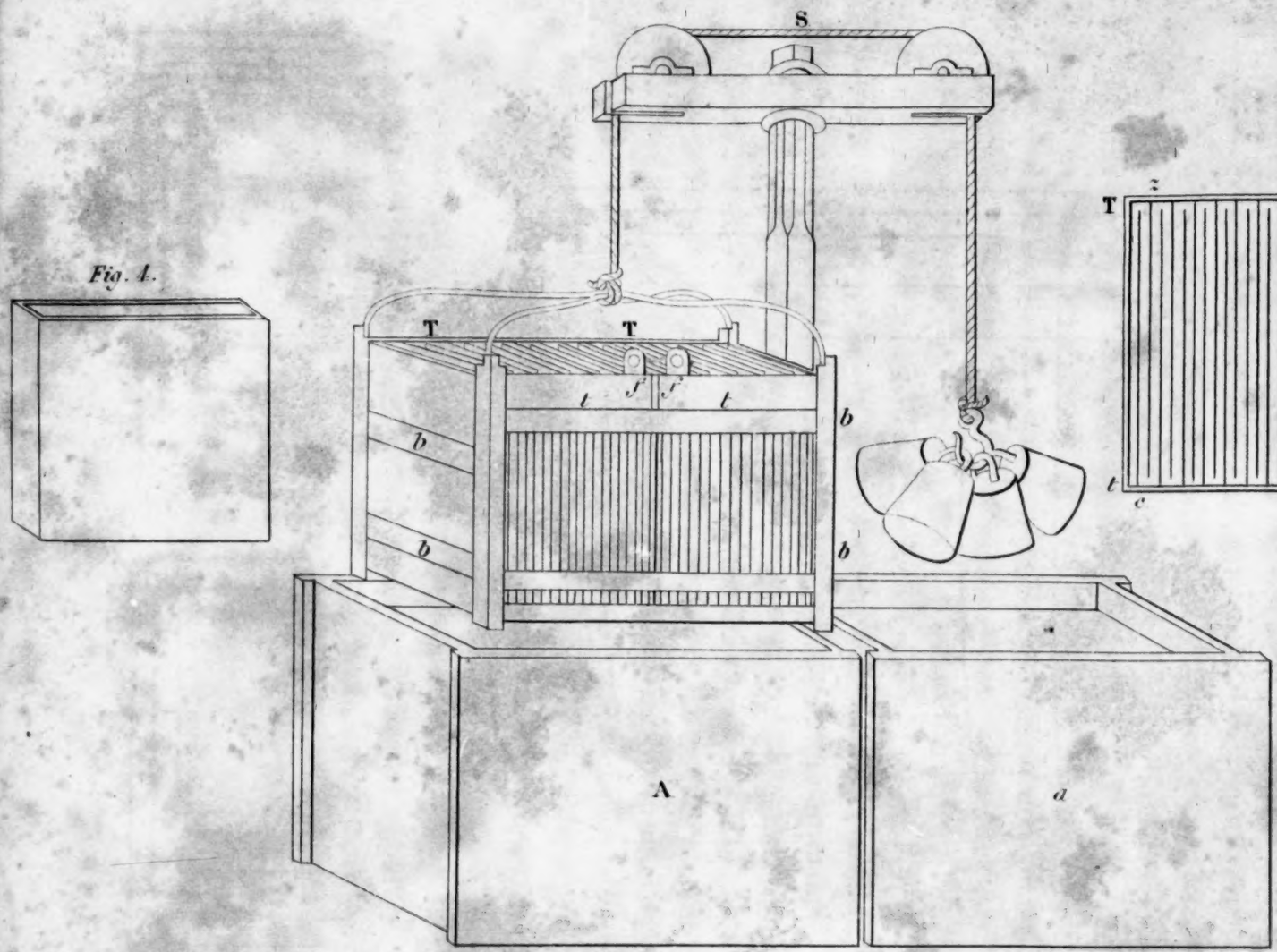


Fig. 4.

Fig. 2.

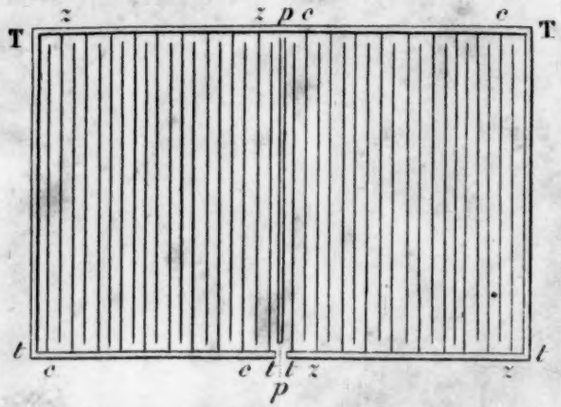
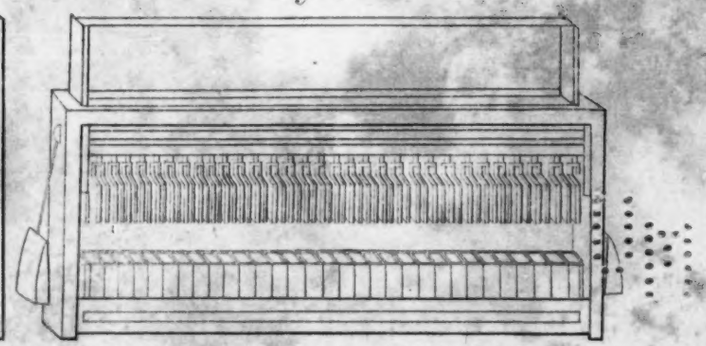


Fig. 3.



Published by R. Desilver N° 110 Walnut St. Philad^a

Fig. 1.

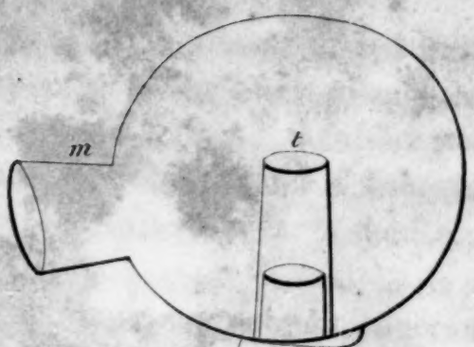
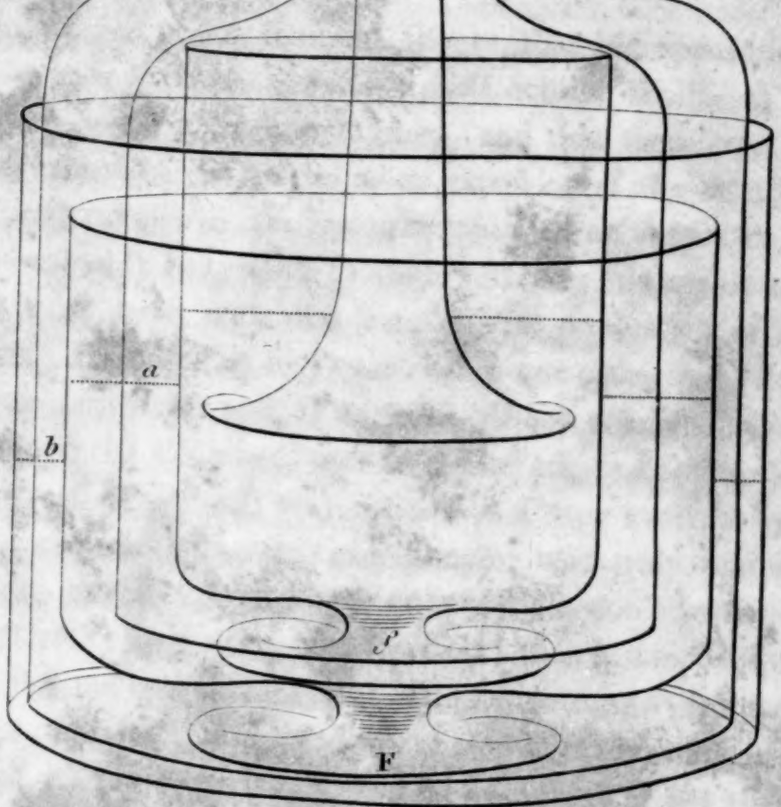


Fig. 4.

Fig. 5.



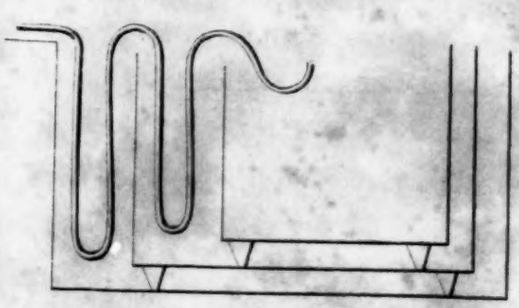


Fig. 3.

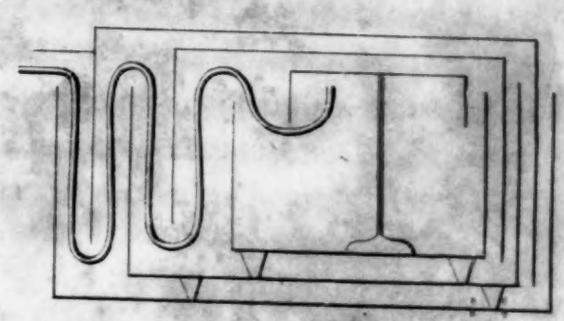


Fig. 2.

Published by R. Desilver N° 110 Walnut St. Phila.ª

Published by R. Desilver, N° 110 Walnut St. Phila.ª

own judgment in the present case, I had consulted several competent gentlemen on the point in question, and that they concurred with me in opinion. I further stated, that I would again examine into the subject, and if I should find myself in error, I would, in a succeeding number of the Recorder, cheerfully correct my mistake.

Having, however, my opinion confirmed, rather than shaken by a second examination of the professor's pamphlet, I again, in reply to some observations of Dr. Hall on this subject, stated my conviction, that his apparatus is in fact and principle, nothing more, than an extension of Wollaston's elementary battery. This opinion, I supported with arguments, which the professor labours to invalidate, in the above reply. Let us see how he has succeeded.

In the first place, then, he alleges, that his apparatus consists of *two pair* of plates in *one recipient*, whilst Wollaston's consists only of *one pair*, in one recipient, and that therefore, his apparatus cannot be considered as an extension of the latter. Let us try this argument. Wollaston constructed an apparatus, in which there are only *two minute* galvanic surfaces; this apparatus evolves an intense heat, without a concomitant production of electrical effect. Dr. Hare at first constructed one consisting of *two large* galvanic surfaces; and, as might have been expected from the experiments of Children, and from the effects of the elementary battery of Wollaston, very intense heat was evolved by it. So far, then, the professor's calorimotor was truly and obviously nothing more, than an extension of Wollaston's battery. They were strictly analogous, both in effect and in principle of construction; the one was an apparatus of *two minute*, and the other, one of *two large* plates. But, he did not stop here; he made a *duplication* of his first apparatus;—that is, he combined *two simple* calorimotors, each having *two* galvanic surfaces, in such a manner, as to produce one, of *four* galvanic surfaces, and capable of being immersed, in the same recipient. By this contrivance, then, he obtained the *combined effects* of *two simple* apparatus, constructed on the principle of Wollaston's battery. It must not be forgotten however that a mere duplication of a *simple* apparatus, is insuffi-

cient to alter the principle of its action, or the analogy of its structure. If the professor were asked, to prove, that a *double barrelled gun*, is not in fact and in principle, an extension of *a pistol*, I am at a loss to know, by what mode of reasoning, he would attempt this, unless it should be, by arguments similar to those which he here employs, in order to prove that his calorimotor is not in fact, and in principle, an extension of Wollaston's battery.

Again he says, "the action of a pair of galvanic surfaces was known, not only before the time of Wollaston, but anterior to that of Galvani himself. Hence, it must be evident, that it is only in *size*, *arrangement*, and *application*, that Wollaston's apparatus can possess originality. But, in all these points, I trust, the characteristics of the calorimotor, are at least equally peculiar." What does this prove? certainly nothing more than that the professor's calorimotor differs in *size*, *arrangement*, and *application*, from Wollaston's. As to its unlikeness to all others in *size* and *arrangement*, I am not at all disposed to dispute. I never did intimate any doubts on this score. I stated explicitly in my remarks, that as a *mechanical* contrivance, I thought it entitled to merit; and I am certain, that the professor will do me the justice to acknowledge, that I have never asserted it to be similar in *size*, to that apparatus, of which I consider it an extension. The fact is, the principal reason why Wollaston's battery was inapplicable to the purpose which Dr. Hare had in view, in the construction of his apparatus, according to his own confession, is, "*that it was too minute to allow him to make the observations he had in view.*" He was therefore obliged to construct an apparatus of a larger size, on the same principle, in order that he might apply it to his particular purposes. It consequently differed in *size*, *arrangement*, and *application* from Wollaston's battery, and cannot therefore, according to Dr. H. be considered an extension of it. Let us try this logic. A. makes a pistol, B. wishes to shoot wild geese; he finds the pistol "*too minute*" for his purpose. He therefore "examines" what effect "*one enormous*" barrel will have—whether it will not make more noise, and carry a bullet further than the pistol. He finds it to do so; but he is still not satisfied; he

seeks to increase the efficacy of his large barrelled gun; he adds another barrel,—he constructs a gun of two "*enormous*" barrels, and it answers his purposes. Now, as this gun undoubtedly differs from the pistol in "*size*, *arrangment*, and *application*," it cannot be considered, according to the dialecticks of the professor, an extension of it. *Quid vetat ridens discere verum?*

As to its *application*, I think I am warranted in asserting, that the *calorimotor* differs in no respect as a *calorimotor* or *heat-mover*, from Wollaston's battery. Both these apparatus are employed to evolve great degrees of heat; and both do so, by virtue of the same law of galvanic surfaces ;—that law, by which the electrical action of a battery is not increased by the increase of the size of the plates, but by the increase of the number of their pairs, whilst the *heat-moving power* is increased with the size of the plates, and the *decrease* of the number of pairs. It is upon the identity of effect, as *heat-moving powers*, depending on identity of principle of construction, and by consequence, on identity of galvanic law, that I predicate the opinion I entertain of the perfect similarity in the essential characteristics of the two instruments, and that I consider the calorimotor of Dr. Hare as differing in nothing from the elementary battery of Wollaston but in external conformation, and that it is manifestly and truly, only an extension of it. He seems to lay much stress on the *object* which Wollaston had in view in the construction of his battery, and the theoretical notions he entertained respecting galvanism. He thinks that, as he himself had a different object in view in the construction of his apparatus, it cannot be considered as arising out of Wollaston's. Can the purposes, for which an apparatus is designed, or the particular theory which it is intended to confirm, alter the nature of its construction, or the laws of its action? If Wollaston's theoretical notions relative to heat and electricity, differ from those of Dr. Hare, does it follow, that the elementary battery of the former, differs in the laws of its action and the principle of its construction, from the calorimotor of the latter? Certainly I may leave him to enjoy the triumph of such logic.

It appears, by his own accounnt, that in consequence of the expe-

riments of Children and De Luc, he was led to examine, whether by extending the principle which was demonstrated to exist by these experimentalists, he might not exhibit heat more purely by using "*one pair* of *plates* of enormous size, or *what might be equivalent thereto. The elementary battery*, he says, "*of Wollaston was too minute to allow him to make the observations I had in view.*" Is this not, in fact, acknowledging that Wollaston's battery wanted but *extension*, or *increase* of size, to render it a fit apparatus for the purposes Dr. Hare had in view? Wollaston's apparatus consists of *one pair* of plates, but they are *too small* to produce sufficient effect for the purposes Dr. Hare had in view. He therefore, very ingeniously, and with vast originality, "was led *to go farther*, and to examine what an apparatus of *one pair* of *enormous* plates" would produce. Having come to this original determination, of using "*two enormous plates*, or what might be equivalent thereto," instead of *two very small ones*, which were "too minute for the observations he had in view," he constructed an apparatus, which he thus describes. "Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half distances from each other. All the plates of the same kind of metal, were soldered to a common slip, *so that each set of homogeneous plates formed one continuous metallic superficies.*" This then was the *first* step in the professor's brilliant discovery. By a subsequent one, he extended this apparatus, so that by bringing two of this first and simple form together, in such a manner as to enable them to be immersed at once, in the same recipient, he obtained the combined effects of two simple elementary batteries. In this, I willingly confess, there is displayed considerable mechanical ingenuity. As a mechanical contrivance, it has no small degree of merit. But I maintain, that, as a philosophical contrivance, it is a direct and obvious derivation from Wollaston's battery. All the essential characteristics of their action and construction are virtually identical. If Dr. Hare's calorimotor produces an intense heat, without a proportionate degree of concomitant electrical effect, it is from the same principle precisely, that Wollaston's battery produces a similar effect. If

the effects of Dr. Hare's apparatus are greater than those of Wollaston's, it is because the former is an extension of the latter.

He asks, "how can an experiment designed for the examination of the heat-moving powers of the largest galvanic pair ever made, be considered as an extension of another, which has for its objects, the minutest arrangement of electrical substances, by which visible ignition can be produced?" How can it not? Surely if *two* "enormous plates" are employed to evolve a powerful degree of heat, there is no contradiction in calling it an extension of another experiment in which heat is evolved by *two* plates of the *smallest* size.

In the following quotation, the professor again unwittingly admits the justice of my remarks relative to his calorimotor. "By *lessening* the original pairs, Wollaston ascertained the curious result, that even when *very minute*, ignition might be effected by it. By *enlarging it*, I sought to shew the phenomena, inconsistent with the idea that the ignition resulted merely from electrical discharges."

Upon the statement I made, in my remarks on the calorimotor, that Wollaston found that heat was powerfully evolved by his elementary battery, without a concomitant production of electrical effect, Dr. Hare remarks: "So far from distinguishing the heat produced by his apparatus, from the electrical effect, Dr. Wollaston considers it as itself an electrical effect." This may be so; but the fact is, that heat was evolved without those effects which are usually denominated electrical or galvanic; and it is only with facts and not with the speculative opinions of Dr. W. or Dr. H. that we have to do on the present occasion.

The professor then goes on and says, the following statement is equally unfounded. "Thornsdorf, Thenard, Fourcroy, Vauquelin, and particularly Children, have demonstrated that the *electrical* action is not increased by the increase of the size of the plates, but by the increase of the nnmber of pairs, and that on the contrary the evolution of heat is encreased with the size of the plates." This statement, I am able to substantiate by the authority of an Editor, "who does not write on what he does not understand,"—one whom I am glad to find Dr. H. respects.

In the 70th, number of the Annals of Philosophy, page 137, we find the following paragraph: "Thornsdorf's discovery of the efficacy of large metallic plates in producing combustion, was the next step in the improvement of the galvanic apparatus. This discovery was verified by Fourcroy, Thenard, and Vauquelin. These gentlemen found that the *electrical* action of the battery was not increased by the increase of the size of the plates, but by the increase of the number of pairs; but the chemical action, as far as *combustion is concerned*, and as far as the decomposition of those bodies which are very difficult of decomposition is increased, with the size of the plates." This is the language of Dr. Thompson, a chemist, "who does not write about what he does not know," and yet Dr. Hare, who I presume writes about nothing but what he does know, says it is unfounded! I will leave him to settle this with Dr. Thomson.

Dr. Hare seems to think, that as an editor of a Recorder of original communications and intelligence in medicine, I have deviated from my duty in offering my opinion on his calorimotor. He moreover insinuates, that I am incompetent to give my opinion upon the subject which I have ventured to discuss. If the professor will look into the prospectus of our Journal, as well as into its contents, he will find that it is, as it has always been, a part of our plan to review books on medicine, and the collateral branches, freely and independently. As to my qualifications to give an opinion on this or any other subject, Dr. Hare must know more of me than he does, before I can allow him to decide.

Rejoinder to a Reply to the Review of Dr. Spalding's Pamphlet on the Scutellaria.

It has ever been a principle with us, to open our pages to free discussion. It was under the influence of this disposition, that we gave a place to Dr. Spalding's reply to the review of his pamphlet; and not, that we consider d his letter as a satisfactory

refutation of the criticisms contained in the review. On the contrary, we approve entirely the sentiments of the reviewer. We, however, regret, that we ever noticed this production, and perhaps some apology is necessary to our readers for having done so. It is simply this : it is not often that it comes within our province to notice the publications of our own country; for numerous as are the authors which she has produced in the various branches of literature, and in other departments of science, she has very few medical writers. For this reason, we always have an eagerness for opportunities of noticing the works of American physicians. And although the literary merits of this pamphlet did not entitle it to any notice, and we considered it as utterly destitute of every quality that could give it a claim to a review in a respectable magazine, we would have been guilty of culpable indifference to the interests of medical literature and science, which, as editors, we are bound to superintend, had we suffered this production, however contemptible, to escape the censure and condemnation it deserved. We could not regard it as a *harmless* book, calculated merely to give a little notoriety to its author's name, and then to perish. We considered it as a book fraught with mischief, and calculated to mislead the profession and the public, on a subject involving the most important interests of both. It pretends to furnish physicians with an infallible remedy for a disease, which nothing but the most prompt and judicious exercise of skill can prevent; and for which no antidote will ever be found, but by the most patient and unceasing investigation. It holds forth to the public a pretended substitute for all the aids of medicine; and persuades them that the profession can give them no security from the occurrence of hydrophobia, and can do nothing for its alleviation after it has supervened. Is all this merely the representation of our fears? or is it not what has already happened? Have we not seen physicians, in seeming forgetfulness of the value of human life, lay aside the prophylactic means which experience has sanctioned as infallible for the prevention of hydrophobia, and substitute in their stead this inert and dangerous remedy? Have we not seen them, notwithstanding its failure as a preventive, resort to it as a means of cure in the very case in

which the experiment had been made? and do we not hear them expressing unbounded confidence in the efficacy of the scutellaria, and declaring their determination to administer it in every case which may come under their care? Have we not too seen unfortunate persons who have been bitten, neglect the only means of averting the dreadful consequence; refuse the advice of intelligent physicians; and commit their lives to the editor of a newspaper, for the purpose of making an experiment with scull-cap?

Dr. Spalding disclaims any belief in the efficacy of this plant; and pretends to have had no other design in publishing this mass of evidence in its favour, than to give a mere detail of the facts connected with its use, and to expose the weakness of the testimony upon which its celebrity was established. But we confidently assert, that the inevitable inference is, that he was a *firm believer* in its virtues; and we venture to say, that any man, upon reading the pamphlet, will arrive at this conclusion. Instead of exposing the absurdity of its pretensions, he mentions not a word of the insufficiency of the evidence; but says, in the tone of triumph and defiance, that not a single case of failure had yet been adduced. How does such conduct comport with professional integrity and honour? Without reposing any confidence in the remedy, he recommends it to the public by a most imposing mass of testimony; and circulates it as extensively as possible, by suffering his pamphlet to be hawked about the streets in the baskets of hucksters.

As to Dr. Spalding's claiming exemption from our criticism, on the ground that his pamphlet was read before the *Historical* Society, and consequently is not a *medical* work; it is very well known to us, that Dr. S. was permitted to read it before that society merely through courtesy; and that nothing but an extreme delicacy of regard for his feelings, on the part of a few members of that body, preserved him from a vote of censure for the liberty he had taken in publishing his book as if sanctioned by them.

But enough. The motives of the author in this publication, are too obvious to escape notice. The efficacy of the scutellaria was yet a matter of doubt. It might, or it might not be found to be a remedy for hydrophobia. This was too tempting an oppor-

tunity to reap all the honour of having introduced a valuable antidote to the notice of the profession, without running the risk of sharing its disgrace should it prove an imposition. The Doctor could not resist the temptation. The character of the article would soon be decided, and some one else might anticipate him, and it was easy to declare that he never had any confidence in the article, should that become necessary. Such conduct is utterly unworthy a member of a liberal profession. But it shows us to what ridiculous lengths a man will be carried by a restless longing after *fame*. It is an *ignis fatuus* that leads its pursuer into many a quagmire. Although Dr. S. accuses us of misrepresenting his statements in some places, we shall not again enter into an examination of his pamphlet; this task would be exceedingly irksome to ourselves, and tedious and unprofitable to our readers. We must be content to refer to Dr. Spalding's pamphlet itself, and to the review of it contained in our number for April last, to convince them that we have done the author no injustice; and that however severe our criticisms may be, they can never equal the censure *he deserves*. D.

Cases of Morbid Anatomy. By John W. Francis, M. D. of New-York.

Professor Francis, of the University of New-York, has just published a work, in quarto, entitled "*Cases of Morbid Anatomy.*" Morbid anatomy is an exceedingly interesting and useful department, as it is from it alone that correct ideas can be acquired of the structure of parts in their natural condition, the effects of disease ascertained, and the nature and seat of diseases determined. Collections of morbid anatomy, by developing the great and various alterations of structure which the organs of the body may undergo in disease, and by affording data whereby to judge of the influence of these changes of organization upon the functions of the animal economy, are calculated to advance the science of physiology, to enlarge the views of the patholo-

gist, and to extend the boundaries of practical medicine. The cases furnished by Dr. Francis, are of a very important and instructive nature. They are rendered exceedingly interesting by the judicious narration of their previous history; and they are detailed with an accuracy and judgment, which must rank this work among the ablest productions in the department to which it relates. The diagnostic indications of a scirrhous and strictured state of the pylorus, have perhaps never before been so well defined; and by no other author has the peculiar appearance of the liver induced by intemperance, as distinct from the effects of ordinary inflammation, been so minutely or so well delineated as we find it here. The work is a valuable acquisition to the profession. The able manner in which it is executed, induces us to hope, that the author, who seems so well qualified to conduct such researches, will continue and extend his pathological inquiries. It is by such investigations, conducted upon the plan of which Dr. Francis has given us so good a model, that medicine is to be perfected. D.

ANALECTA.

Observations on the Properties of the Blood.

Article 1st, Royal Society, November 11th, 1819. The conclusion of a paper was read, detailing the results of some further researches of Sir Everard Home on the properties of the blood. He has, he considers, discovered that there exist in that fluid globules which are smaller and of a different nature from those it is commonly supposed to contain. They were first observed by Mr. Bauer, whilst examining the layers composing an aneurismal tumor. They were seen in the coat in contact with the circulating blood, in the proportion of one to four, compared with the larger globules; but in the other layers they were more numerous, and in these which have been first formed they existed in the proportion of four to one. Mr. Bauer estimates their size $\frac{1}{2800}$ of an inch. Crystals of muriate and phosphate of soda, and phosphate of lime, were found in making a section of another aneurismal tumor. Sir Everard Home considers that those globules existed originally in the serum; the globules being to be seen only after the blood has coagulated. In coagulated lymph, formed during intense inflammation, these globules were found mixed with a few colourless blood-globules. They were also found in great numbers in the upper firm coat of the buff of the blood, while the lower and softer parts consisted chiefly of red blood-globules. He proposes to call the new globules by the name of globules of lymph, to distinguish them from the red blood-globules. Sir Everard Home has also found, that the quantity of carbonic acid gas evolved from buffy blood under an exhausted receiver, is much less than that from healthy blood,

and that by far the greatest quantity of this gas was yielded by blood drawn from a healthy person an hour after a full meal. Both lymph globules and blood globules were found in the mucus of the pylorus and duodenum. In chyle, the size of the globules is various.

New mode of treating Nævi Materni.

Several cases of this affection have lately been successfully treated in Germany by inoculating the part with vaccine virus.

A child had on the right cheek a nævus of a deep red colour, about a quarter of an inch in diameter. Some vaccine virus was introduced under the cuticle of the affected part: on the ninth day, a very large vesicle had formed, and the surrounding inflammatory areola covered almost the whole of the cheek. At the end of a month, the nævus had disappeared, a slight mark of a pale reddish colour only remained.

A nævus of the size of a shilling, of a deep blue colour, situate near the axilla, was inoculated with vaccine virus by Dr. Burkhardt: eight vesicles formed, suppuration became established, and the nævus disappeared.

Six cases of nævus were treated in the same way by Dr. Klein. In the greater proportion of them, a red areola appeared, which continued until the desiccation of the pustules; on the falling off of the crusts, a regular cicatrix was discovered, and the nævi had disappeared. Dr. Muller had seen this mean successfully used in nine cases. Lond. Med. Phys. Journal.

Portable Sudatorium.

The following is the description of an instrument called a Portable Sudatorium, by its supposed inventor, Charles Gower, M. D. Fellow of the Royal College of Physicians, and Physician to the Middlesex Hospital. This is noticed by Dr. Johnson in his April number of the Medico-Chirurgical Journal with some very handsome compliments to the modesty and ingenuity of the contriver.

"The sudatory cradle is made of longitudinal bars of osier, placed at the distance of an inch or more asunder, and preserved in their situations by occasional cross-bands of basket-work. Its length is about four feet, and main width, at the base, which covers the patient's shoulders, two feet, the smaller end about a foot and a half. It may thus be compared to the half of a truncated cone, divided in a direction from the apex to the base. Or it may be compared to the cradle which is placed over a fractured limb, to keep off the weight of the bed-clothes. Within the edges of the narrow end is laced a thin piece of board, with a circular orifice in its centre, for the admission of the point of a curved tube, through which the warm vapour is to be conveyed. This curved tube is composed of sheets of tin-plate, grooved, but not soldered, to prevent the joints from being separated by the heat of the lamp placed under its wider extremity. The tube is about thirteen inches long; the larger opening or end is three inches in width; the smaller, one inch and a half. It is in shape resembling a powder horn. It has a few holes in its sides, near the wide extremity, to admit the cool air from without, as the lamp rarefies the air within. The lamp itself ought to be grooved also, and not soldered, as it has a considerable degree of heat to fear. It is merely a spirit of wine lamp, with three burners."

Those among our southern brethren who have been long familiar with Dr. Jennings's "Patent vapour bath," which has been generally known as a domestic remedy for years in Virginia and Maryland, will smile at the minute description of an old acquaintance which is here given by this "modest and ingenious gentleman." We should be sorry, directly or indirectly, to countenance the belief that we think it decorous in a member of a liberal profession either to conceal or obtain patents, for any inventions or discoveries which he may have made tending, as he supposes, to diminish the mass of human suffering. But though we would deprecate such means as degrading to our profession, yet we cannot but view with still greater disapprobation such an unblushing assumption of the merit of another, however humble it may be.

Kadmium.

Some observations made by Dr. Rosenbaum, have tended to prove that the efficacy of Zinc in the cure of diseases is entirely owing to the kadmium it contains. This is a new metal, which Professor Stromeyer of Gœttingen has lately discovered to exist in the sulphuret of zinc, and which may be obtained in greater or less quantity from all its officinal preparations. Dr. R. has used with great success the sulphate of kadmium in opacities of the cornea, in which practice he has been imitated by Professor Himly of Gœttingen and Dr. Wedemeyer of Hanover.

Blood-vessels of the Retina.

Dr. Sœmmering has discovered by means of the camera lucida of Wollaston, affixed to the microscope of Dollond, in some very minute injections of the eye, that the retina is supplied with a peculiar set of vessels from the arteria centralis, which terminate in the vein of the same name, and these, he says, have no communication with the vessels from the choroidea, excepting, perhaps, at the anterior margin of the retina. This want of connection between the two coats, explains the reason, why the retina is oftentimes found corrugated, not only after death, but after violent inflammation, contusion, wounds, &c. of the eye.

Comparative Anatomy of the Eye.

Dr. William Sœmmering, son to the celebrated anatomist of this name, has lately published a comparative anatomy of the eye,* illustrated by a series of plates, displaying the horizontal section of a number of eyes of different animals. The author has not confined himself in this work to the specimens only, afforded him in the rich cabinet of his father, but has availed himself of those of Blumenbach, Cuvier, Meckel, Albers, Rudolphi, and others. From the patience, labour and talent, evinced

* De oculorum hominis animaliumque sectione horizontali commentatio, &c. pp. 79 in folio, Gœtting. 1818.

in this first undertaking of Dr. S., and from the accuracy and beauty with which the drawings are executed, we deem this work no inconsiderable acquisition to our stock of medical literature.

Effects of Sulphuric Acid on Wood and Linen.

Dr. Vogel, of Munich, has submitted to the Academy of Sciences at that place, an essay on the discovery of Mr. Braconnet of Nancy, in which he confirms the account given by that gentleman, of the effects of concentrated sulphuric acid on wood and linen. He details some additional experiments on other vegetable productions, such as paper and straw; and says, that by diluting the sulphuric acid with a due addition of water, saw-dust, cut linen, &c. may be converted into gum and saccharine matter.

Sugar of Lead in Yellow Fever.

Dr. Irvine, of Charleston, S. C. has just published a treatise on Yellow Fever, in which he recommends the *acetate* of *lead* to compose the irritability of the stomach, attendant upon that disease, even after black vomit has come on. It will be seen in our number for July last, page 350, that Dr. Davis of Columbia, S. C. has exhibited the same article in irritable states of the stomach attendant upon bilious fevers. He begins with three grains at a dose, and repeats it when necessary.

Muscular structure of the Urethra.

Sir E. Home read a paper the 1st of June, entitled Microscopical Observations on the Urethra. Sir E. H. is of opinion from observation made by Mr. Bauer, with the microscope, that there are muscular fibres in the structure of the urethra. He observes that there is an internal membrane, on an external covering of muscular fibres; on the surface of the former mucous folicles open. The latter is made up of short interwoven fibres, forming fasciculi, united by an elastic substance of the consistence of mucus. Ac-

cording to the Bart. these facts go to prove the fallacy of the opinion, that the lining of the urethra consists of circular fibres, and thus throw a new light on stricture. A spasmodic stricture being caused by a small portion of the longitudinal fibres, while the others are relaxed; and a permanent one by the exudation of lymph from inflammation.

The same gentleman read another paper to the Society, on the 15th of June, suggesting an improvement on the high operation for lithotomy, recommended by Dr. Souberbielle and Mr. Carpue. Instead of making a wound into the perinæum, as directed by these gentlemen, for the purpose of allowing the urine to be evacuated, as it enters the bladder, Sir E. H. proposes that a flexible gum catheter should be introduced *per urethram*, and allowed to remain until the wound above the pubis be healed. One case is given, where the operation was performed in this way, with success.

ROYAL ACADEMY OF SCIENCES, PARIS.

M. Portal read a paper, to prove that congenital blindness and deafness, arises occasionally from the fœtal structure not disappearing after birth. That the former may depend on the membrana pupillari not being torn, and the latter on the mucous secretion still remaining in the meatus auditorius externum. M. Chretien, of Montpellier, has sent to the academy, a voluminous work on the employment of gold in medicine. The committee report that, by rubbing the triple muriate of gold and soda, on the tongue, they have been able to cicatrize scrophulous ulcers, to resolve syphilitic swellings, to destroy nodes in part, to stop the progress of cancers, &c. &c. (These observations we would advise our readers to take with considerable latitude.)

A decoction of parsley (apium petroselinum) is an admirable lotion for the *Erythema Mercuriale*. It was the favourite prescription in these cases, of a distinguished southern practitioner, now dead, who was remarkably successful in the treatment of this disease.

INDEX.

A

Page

Arteria Innominata, how to cut for it . . . 27
A new method of removing deformities and defects arising from cicatrization . 58
A new method of treating encysted tumours . . 61
Ascites 107
Asthma 147
Aneurism, two cases of . 193
Asclepias Tuberosa . . 334
Arum Tryphillium . 335
Adulteration of malt liquors in England . . 337
Anteversio Uteri, remarks on Dr. Granville's Case . 452
Abdominal integuments . 491
Arsenic, its use in Chorea . 454
Agrimony 134
Atomic theory, exposition of 64
Auscultation mediate . 534

B

Brachial artery, on the Surgical anatomy of it . . 46
Burgon on certain articles of the Materia Medica . . 331
Bronchocele, observations on 423
Blood-letting, on its modus operandi 461
Blood-vessels of the retina . 632

C

Carotid Arteries, anatomical description, how to cut for them 36
Caustic Potash, its efficacy when applied to the spine in the treatment of Tetanus . 170
Common Iliac Artery, wound of by Professor Gibson . 185
Cholera Infantum, treatment of 335
Contraction of Limbs from burns, best method of removing them 341
Cephalia, a case of . . 411

Page

Chorea, on the use of arsenic in it 454
Cataract, observations on the various modes of operating for its cure 476
Cataracta dura, observations on 478
Caseosa, seu gelatinosa 479
Lactea, seu Morgagniana . . 480
Capsularis, seu Membranacea . 480
Acreta . . 482
Chapman's, Dr. Therapeutics, notice of 131
Chimaphila Maculata . 333
Cholera Infantum, by Dr. C. D. Meigs . . . 498
Cooper's, Dr. T. mode of preparing Prussic Acid . 575
Correspondence between W. Philip and B. C. Brodie . 579
Calorimotor, Dr. Hare's . 605
Congenital Deafness, some observations on by Portal . 636

D

Duncan's Reports of the Practice in the Clinical wards of the Royal Infirmary of Edinburgh, &c. 73
Dean on the use of Mercurial ointment in Erysipelas . 299
Dentition, remarks on . 334
Ducachet, on the modus operandi of Blood-letting . 461
Dewees's, Dr. essay on Parturition, notice of . . . 133
Ducachet, Dr. describes an extraordinary case of maleformation of the genital organs . 415
Dilatation of the bronchiæ . 541

E

External Mammary artery, how to be taken up . . 47
Epigastric artery, how to be taken up 47

Page

External Iliac Artery, how to be taken up 48
Extract of a letter to Editor from Dr. Albers of Bremen . 134
Extract of Hemlock . . 144
Erroneous notions of duty in insanity 308
Eupatorium perfoliatum . 333
Eczema Rubrum, case and remarks on 453
Encysted tumours, treatment of 61
Egophony 542
Emphysema of the Lungs . 545
Eberle's, Dr. J. replies to Dr. Hare on the Calorimotor . 620
Eye, comparative anatomy of it by Dr. Sœmmering . . 634

F

Fascia of the Prostate, a description of 9
Femoral artery, how to cut for it 49
Fowler's solution in nodes, by S. Colhoun 347
Frick, Dr. G. on the treatment of Cataract . . . 476
Francis' Morbid Anatomy . 629

G

Gluteal and Ischiatic arteries, mode of taking them up . 48
Gastric worms, by Joseph Klapp, M. D. . . . 155
Gillingham on the effects of pressure on tumours of the breast 166
Gillingham on erroneous notions of duty in insane people . 308
Gibson's, Dr. Case of a wound of the common Iliac Artery . 185

H

Hare's Dr. new Theory of Galvanism 149
Hydrops Capitis Infantum, by J. C. Rousseau . . . 315
Home's observations on the blood 631

I

Infiltration of urine, frequently the cause of death in Lithotomy 12
Internal Mammary Artery, how to be taken up . . . 47
Internal Pudic Artery, how to cut for it 49
Intestinum Cæcum, separated and discharged per anum . 54
Incision of the Structure of the Urethra . . 289
Jameson on the prostate fascia 325
Ileus, observations on . . 424

K

Klapp on Gastric Worms . 155
Kadmium . . . 634

L

Lewis on the use of Caustic Potash in Tetanus . . 170
Laryngitis, a case of . 418
Lithotomy, new mode of operating 449
Lumbrici Teres discharged through the abdominal integuments, a case related by P. Macaulay, M. D. . 491
Ligature of the Arteria Innominata, remarks on by Dr. Mott 514

M

Malignant Fever of the U. S. and West Indies, in 1819 . 203
Mercurial ointment in Erysipelas 299
Muriat of Mercury in Erysipelas 305
Mercurial Ointment in the puriform discharge of the ears 307
Marasmus . . 335
Macaulay, Dr. P. relates a case of lumbrici teres discharged through the abdomen . 491
Meigs, Dr. C. D. on Cholera Infantum . . 498
Mercurial Ointment, new mode of making it . 509
Mott's, Dr. Valentine, Remarks on the case of ligature of the Arteria Innominata . 514
Maleformation of the genital organs, a case by Dr. Ducachet 515

N

Nice, on removing contractions in limbs from burns . 341
New method of preparing extracts 143
Nævi Materni, new mode of treating them . . . 632

O

Observations on Lithotomy, by G. S. Pattison, Esq. . 1

	Page
On the practice of medicine among the Hindoos	282
Observations on certain native medicinal vegetables, by J. Burgon	331
On the Prussic Acid, by Thomas Cooper	349
Ophthalmia, description and mode of cure	413
Ophthalmia Periodica, by J. Bostock	417
On Cholera Infantum, by C. D. Meigs	498
Officers and Members of the Philadelphia Medical Society during 1819-20	290

P

Popliteal Artery, how to take it up	50
Peroneal Artery, how to take it up	51
Puerperal Convulsions, after delivery, a case by Dr. F. Strein	53
Pattison's two cases of Aneurism	193
Podophyllum Peltatum	331
Prunus Virginiana	333
Peritonitis, remarks on a case of	447
Prostate Fascia, observations on by H. G. Jameson	325
Pectoriloquism	539
Prussic Acid, new mode of preparing it	575
Parsley a good remedy for Erythema Mercuriale	636

R

Results of the examination of the body of A. Mitchel	57
Review of Dzondi's Contributions, &c.	57
of M'Nevin's Exposition of the Atomic Theory	64
of Duncan's Reports of the Clinical Wards of the Royal Infirmary of Edinburgh, during the months of Nov. Dec. Jan. 1817-18 and May, June and July 1818—	
of Clutterbuck's observations on the prevention and treatment of the Epidemic Fever in Great Britain—	
of Percival's practical observations on the treatment and prevention of Typhus Fever—	
of J. V. Edler and Hildebrand on Typhus Fever	73
of Curtis' Treatment of Puriform discharge from the ear	105
of Scarpa on Pregnancy accompanied by Ascites	107
of Dickenson's observations and examples of Plegmasia Dolens, &c.	114
of Blackall on Dropsy	239
of Pattison's paper on Lithotomy	252
of Reese's observations on the Epidemic as it prevailed in Baltimore	260
of Spalding's History of Scutellaria	267
of Chapman's Elements of Therapeutics	273
of Bache's Chemistry,	372
1. Essay on Hydrocephalus Acutus, by Dr. Cheyne. 2. Hydrocephale Argue, par M. Vaidy. 3. Memoire sur l'Hydrencephale, par J. F. Coindet, M. D. 4. A statement of the early symptoms of Water in the Brain, by G. D. Yeates. 5. Of Apoplexia Hydrocephalica, by J. Cook	385
of Practical Medicine	411
Medico-Chirurg. Transactions, Vol. X.	517
Memoir concerning the effects of the actual Cautery, by L. Valentin	526
Revue Medicale, Historique and Philosophique	534
A memoir on Fistula lacrymalis	547
A memoir on Fistula in Ano	544
Observations Pratiques sur les fumigations Sulfureuses. Par J. de Carro	550
of Blane's Medical Logic	558
of Magendie on Prussic Acid	566

	Page
Review of Dr. Whilldin's Essay on Dropsy	590
of Meckel's German Archive for physiology	576
Records of Mortality	135
Rousseau on Venereal complaints	171
Revere's account of Yellow Fever in Baltimore	214
Rejoinder to a reply to a review of Dr. Spalding's pamphlet on Scutellaria	624
Remarks on Hare's Calorimotor	280
Rousseau on Hydrops capitis infantum	315
Reply of G. S. Pattison to certain oral and written criticisms	351
to a Review of Spalding's Essay on Scutellaria	381

S

Surgical Anatomy of the Arteries, by George M'Clellan	25
Subclavian Arteries, how to take them up	43
Sketches on Venereal complaints, by J. C. Rousseau	171
Separation and discharge of a piece of intestine, eighteen inches long	54
Selections from Ward's History of the Hindoos	136
Sulphureous fumigation	145
Sudden Death in the latter part of the ninth month of Pregnancy, by Dr. J. Revere	512
Sthethoscope	536
Sudatorium, portable	632
Sulphuric acid, its effects on wood and linen	635
Sugar of lead in yellow fever	635

T

Tibialis Antica, how to cut for it	50
Tibialis Postica, how to cut for it	51
Tetanus, successfully treated by Dr. N. W. Worthington	55
Tumours of the breast, effects of pressure on them	166
Tinea capitis, cured by Chimaphila Maculata	333
Tetanus, a case successfully treated	458

U

Uric Acid	148
Use of Mare's milk in Tænia	143
Urethra, its muscular structure	635

V

Vertebral Arteries, on securing them with a ligature	41
Volvulus, cases of	434

Y

Yellow Fever in Baltimore, during 1819	214
Yellow Fever, sugar of lead used in it	635

Z

Zanthoxylum Fraxinifolium	333

GRADUATES.

In the list of Gentlemen who received the degree of Doctor of Medicine, published in page 293, of this volume, the following names were omitted, viz:

Jonathan D. Price,	*New Jerssy*,	Abstinence.
David C. Skerrett,	*Pennsylvania*,	Modus Operandi of Mercury.
Robert J. Clarke,	do.	Iritis.
Job G. Oslere,	do.	Ergot.
John R. Grigg,	do.	Intermittent.

Thomas Flanners (of Ohio) Thesis, was on Secondary Hæmorrhage.

END OF VOLUME III.